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# Psychological Bulletin

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# Psychological Bulletin

## A CONSIDERATION OF SOME CONCEPTUAL TRENDS IN COMPARATIVE PSYCHOLOGY<sup>1</sup>

T. C. SCHNEIRLA

*The American Museum of Natural History*

### HAVE WE A COMPARATIVE PSYCHOLOGY?

One might say that ideally comparative psychology exists when the comparative methods of science are applied to psychological problems. This must have been, in part at least, what Wundt had in mind in writing his *Vorlesungen* . . . (181). Potentially, such a program would appear to offer a good basis for bringing together and integrating too-divergent fields such as animal, child, and social psychology. Of course this type of approach has not disappeared altogether from psychology, for in recent years we have had an American edition of Werner's *Comparative Psychology of Mental Development* (171), utilizing animal, child, cultural, and social evidence in dealing with common psychological problems, and there is developmental psychology (103) which offers a somewhat limited and loose kind of alliance of child and animal study. However, the pre-contemporary and contemporary trends have led us from such integrative advances, for the most part, undoubtedly in some measure because of the difficult and tenuous nature of extrapolations among these disciplines. On the whole, comparative psychology is currently regarded as contained by animal psychology. But the digestion and assimilation have been uneasy and incomplete at best.

Although a sound use of the comparative method would be expected to unify and mutually strengthen all psychological fields having to do with problems of development, we have at the present time no articulate and integrated advance of the kind. One's strong impression is that no forward-looking program of the kind extends through animal psychology. Konrad Lorenz, the central figure in a vigorous contemporary group of European students of animal behavior, has commented as follows upon one symptom of our situation:

<sup>1</sup> The basis of a paper delivered by invitation at the symposium on Conceptual Trends in Psychology, annual meeting of the American Psychological Association in Chicago, September 1, 1951.

Since the days of Charles Darwin the term "comparative" has assumed a very definite meaning. It indicates a certain rather complicated method of procedure which, by studying the similarities and dissimilarities of homologous characters of allied forms, simultaneously obtains indications as to the phyletic relationships of these forms of life and as to the historical origin of the homologous characters in question. I need not enlarge on the details of this method which is a commonplace to biologists and physiologists. We all know perfectly well what we mean by "comparative" anatomy, morphology, physiology, and so on. But it is all the more misleading if psychologists, who evidently are not familiar with what we mean when we speak of the comparative method, apply the same term in a very loose sense to all behaviour studies concerned with different forms of life. I must confess that I strongly resent it, not only from the terminological viewpoint, but also in the interests of the very hard-working and honest craft of really comparative investigators, when an American journal masquerades under the title of "comparative" psychology, although, to the best of my knowledge, no really comparative paper ever has been published in it (87, pp. 239f.).

If this somewhat abrupt comment is even partially correct, we have cause for concern. If the *Journal of Comparative and Physiological Psychology* does not live up to the senior term in its name, there may be cause for editorial study as to how far the journal *can* be representative under its present editorial program. But since this journal undoubtedly is representative of current developments to an appreciable extent, reactions such as that quoted above must be given very serious consideration.

Let us define the feasible limited objective: a comparative psychology of animal behavior, as the study of similarities and differences in adjustive capacities and personality among the types of living organisms (128). To what extent, if at all, do we have such a subject in American psychology? There appears to be no waning of interest in carrying out studies with animal subjects. In the decades from 1888 to 1940, in the annual total of articles appearing in 14 psychological journals, the percentage of articles dealing with animals below man has risen steadily from 3.5 to 15.2 per cent (20). Our deviations, whatever they may be, evidently cannot be attributed to any neglect to carry out investigations with lower animals.

The answer may lie in how and to what extent we study animals. A few years ago (127), I compared the *Journal of Animal Behavior* in its content from 1911 to 1918 with the *Journal of Comparative Psychology* in its content from 1938 to 1941, as to the kinds of animals used as subjects for the reported studies. A striking difference appeared, typified by the fact that while papers on invertebrate animals fell from 33 to 5 per cent, papers on the rat rose from 19 to 66 per cent. No recent change in this trend is apparent, for Beach (13) found later on



that in 1946 and 1948, respectively, rats were the subject of 72 and 66 per cent of the papers in the *Journal of Comparative and Physiological Psychology*, all submammalian animals just 4 and 6 per cent. This trend toward concentrating on a single mammalian species as subject in the investigations of animal psychologists cannot be taken to mean that we have solved even the principal problems concerning inframammalian animals, notwithstanding the fine beginnings made earlier in this century by Yerkes, Jennings, Thorndike, and our other pioneers in the comparative field. Of course, in this space we cannot hope to offer any substantial analysis of the complex developments in biology and psychology, not to speak of the world in general, which must underlie the described symptoms. The practical consideration of paramount importance here is not how many types of animals we use in our investigations, but whether or not we use the comparative methods of science in working with them. Or perhaps the more primary question is whether we are even interested in such a goal.

Actually, we have developed no discipline of comparative psychology, either in investigation or in theory, of any substantial proportions. Just last year, a scientist in another field was prompted to write that "The life sciences and the sciences of the minds of both animals and men have been neglected too long" (174). Of course there has been a considerable amount of investigation of special problems such as sensory processes, neural mechanisms in learning, problem solving, and higher processes (48, 92, 101, 102, 128, 165). The rat psychologists must not be held lightly, for they have accomplished many useful things (27). But their work has not favored, and in many ways has opposed, a trend toward a consistent, comparative study of behavioral adjustments and psychological capacities throughout the animal series. This is true notwithstanding the fact that the studies on animal capacities carried out by psychologists such as Thorndike, Yerkes, Watson, Lashley, Hull, and Tolman, to mention some contrasts, have occupied a basic and indispensable position in the development of modern American psychology.

However, the fact is not at all obscure that American psychologists have turned from comparative method to an anthropocentric interest in exploring specific problems, from more naturalistic procedures and attitudes to a technical, instrumental emphasis in pursuing these problems. Here we may consider a bit of human behavior which seems not unrepresentative of the general situation. Just last year at a conference, one of our well-known animal psychologists, speaking under the title "Levels of Integration along the Phylogenetic Scale—Learning Aspect" (45), explained that he would limit his discussion mainly to

primates, because they are closest to man and well studied. Now primates are fine animals, interesting and important, but in a comparative sense they are not a very large part of the phyletic baseline. This attitude, we must admit, is typical, a sign of the times and no reflection on zeal or intelligence. In actuality, American psychology has turned sharply away from the beginnings of a comparative methodology perceptible earlier in this century.

Presumably, the broad responsibility of psychologists as *psychologists* concerns investigating all aspects and relations of behavior, personality, and social organization in the universe. Instead, with respect to animal investigations, the following anonymous statement seems to be the consensus:

The principal if not exclusive problem of psychologists is learning about man and his psychological properties and putting this knowledge to good use. We learn most about man by studying man; however, lower animals closest to man can be used advantageously as subjects, because more conveniently obtained and manipulated. They are then used as substitutes for human subjects, and are considered equivalent to human subjects in the situation and problem of study.

This is assuredly a pivotal point in investigative and theoretical attitude, which certainly has led to much valuable knowledge for psychology in general. Some of the underlying assumptions, however, may not be as defensible as seems to be generally assumed. On the one hand, it seems manifest from a large body of sound experimental findings that, for many basic problems in reception and other areas in physiological psychology, rats may serve very well in place of human subjects. But many psychologists, for example Tolman (161) and Hull (61), largely take for granted the rat's equivalence to man as subject for many presumptive psychological problems, and especially for learning.

This conclusion is not foregone, as Köhler (68) has reminded us recently, although first admitting that "Even the rat may reveal great secrets." However, he then says, "But essential characteristics of man are barely discernible in the modest rodent; and what is barely discernible will easily be ignored. So . . . a selection of subjects for reasons of method gradually turned into a selection of special material evidence. . . . Knowledge has been and is being gained by separate rushes which aim at quite particular goals and for the time being at not much else. . . . Take the conditioned reflex, a notion about which scientific bias actually threatened to center." Köhler believes, however, that a "strong interest in the conditions of experimental proof," which he finds dominant among American psychologists, will overcome the hazards, and also

that a trend toward a widened horizon of investigative and theoretical interest is apparent. Can we hope that such is the case?

It is very probable that a strong inclination at present discernible among general psychologists in the United States, to doubt the significance and even the relevance of animal evidence for human psychological theory (e.g., 7), is in good part a reaction to the conditions referred to by Köhler in the above statements. These critics, from a strictly anthropocentric standpoint, do not seriously accept the post-Darwinian premise (165, 166) that studying the animal "mind" is an unfulfilled responsibility of scientists. Not long ago Gordon Allport (7) gave explicit utterance to such a view, particularly on the ground that the gap in psychological capacities between man and his brute relatives is too great for any real help to be expected from infrahuman studies. Far from strongly negating such narrow views, most American animal psychologists at present seem to be *really* nonevolutionary minded, in the sense that they show no special zeal to find how man differs mentally from lower animals and vice versa, but rather focus strenuously on general problems without much attention to phyletic lines.

This practice seems dangerous to the depth and breadth of scientific behavior studies, even for the limited goal of understanding man. Even to justify using animals such as the rat, in a completely anthropocentric sense, as equivalent to man in given respects, there would have to be a substantial use of comparative techniques. How else could we demonstrate that whatever animals might be convenient to use were not too dissimilar mentally from man to meet the specific needs of the problem? Really, the entire discussion here seems rather schizoid to the present writer.

Criteria such as those now prevalent, emphasizing phyletic similarities and minimizing differences, may perhaps be taken to represent a contemporary form of anthropomorphism which subtly avoids mentalistic terminology but inevitably becomes loaded with equivalent implications. The point, of course, is not any serious encroachment of the specific superficialities of traditional mentalism, for the attitude of contemporary American animal psychologists is avowedly objective and *non-mentalistic*. Indeed, the specific superficialities of traditional mentalism are seldom noted. To the arguments of European phenomenologists such as Bierens de Haan (16) for a subjective and inferential description of animal "mind," we may oppose the comment of Boring (19) with respect to one contemporary objective system, that of Skinner (143): "The statements of functional dependency that result from such experimentation leave the concept of a controlling mind in the

same limbo to which phlogiston has been consigned." Quite so! But, all the same, the mentalists may argue, with reason, that the operational and operant systems have made no great strides toward solving the problems of "mental organization" toward which the disowned concepts were directed. A useful hint for objective theorists may be found in Lashley's (77) remark that "the question 'What is the mental state of an animal?' means then: what is the level of organization of its activities." Statements such as this should be inspiring to those interested in the development of a comparative psychology (although they will not satisfy the minority of phenomenologists in our midst). For a discipline of serious contrasts is implied, with emphasis upon the significance of differences as well as similarities in capacities and adjustment patterns. But a scanning of the panorama of current American psychology suggests that we are not too close to this ideal. To investigate, perhaps the most efficient procedure is to undertake a brief comparative study of how some of our chief concepts are used contemporaneously.

#### "THE INNATE"

In a symposium on heredity and environment (157) published in the *Psychological Review* in 1947, Beach (12) says, "In agreeing to discuss any aspect of the so-called heredity-environment problem one naturally infers that the artificiality of the implied dichotomy is obvious to everyone." Unfortunately, this natural inference did not hold even in that symposium, for in summarizing Hunter was able to say: "It will come as a surprise to many psychologists that all five of the distinguished contributors to this symposium have emphasized the role of heredity in the determination of behavior" (157, p. 348). Now does "artificiality" in the first statement mean only that the dichotomy is "man-made," or that it is "man-made, and spurious"? If the latter alternative is the truer of the two, the surprise must be all the greater. It is the opinion of the writer, in his present somewhat uneasy role of trend-spotter, that a really weak dichotomy was accepted implicitly in the symposium. For the contributors agreed in general that the "anti-instinct writers" (55, 71) of the twenties went too far; then, in general, both by implication and by explicit ideology, they swung strongly toward the opposite extreme. We may inquire how far this procedure is a function of new evidence and of valid theoretical advances.

From the general literature one may gather that the aforementioned symposium did represent certain general trends toward a positivistic and recurrent emphasis upon behavior patterns as innate. "Instinct" certainly had the leading conceptual role. One of the contributors,



in discussing whether so-called hereditary behavior is "innate or acquired," made the following statement:

Adult rats, fully watered and fed, will hoard five to twenty pellets per day if placed in hoarding apparatus and left there. Since the rat only eats one or two pellets a day, this is plainly hoarding. And since it comes out spontaneously without training, it is plainly instinctive. Of course, rats may have learned to hoard early in infancy because of competition for food in their home cages, but hoarding is so universal in laboratory rats, always fully watered and fed, that such learning is unlikely (100, p. 336).

Certainly, many animal psychologists have not ceased to rely upon the criterion of universality to bolster an argument for pattern innateness, despite lessons from the past such as that conveyed by the "coenotrope" concept of Smith and Guthrie (146). For that matter, the main load of argument also frequently rests upon the criterion of early appearance, with experiential factors rejected out of hand if there are no obvious indications of them or if the experimenter has not introduced them programmatically (i.e., operationally). And in this exceedingly controversial field, teleological implications frequently lurk under the guise of supposedly functional terms. It may be suggested, for instance (63), that pulling-in or dragging-in behavior would be a proper operational substitute for the possibly improper term "hoarding," at least until we know more about what the rat's intentions are in such behavior. Retrieving behavior is also recognizable as a kind of "pulling-in" activity; and a common use of this term suggests the desirability of investigating possible relationships between these patterns.

There is no denying that much of our contemporary literature in this field is set along nativistic and preformistic lines. But also, as Anastasi and Foley (8) point out in a timely critique, psychologists often (and biologists not infrequently) "define heredity indirectly, vaguely, or inconsistently, especially when it comes to the domain of behavior phenomena." As these authors say, the definition of both heredity and environment, especially for the purpose of psychological investigation, is exceedingly difficult. They offer a provisional consideration of behavior etiology in terms of structural and functional factors instead of heredity-and-environment. How far such substitute dichotomies may be expected to reduce the fallacies in the hoary one of nature and nurture is not too clear. However, Anastasi and Foley believe the substitute should promote general understanding of statements such as Jennings' (65) that: "That which is directly inherited . . . is the set of genes, with the accompanying cytoplasm," in relation to Holt's (55) reminder that "No potential character ever is 'already



contained' in anything . . . [so] that the applicability of the concept of heredity to behavior phenomena is indirect and remote." How well have the implications of statements such as these, based on sound theory in modern genetics, received consideration from animal psychologists and behavior students on either side of the Atlantic, in their investigations of so-called "innate behavior"?

The implication that "beyond the genes, which are the really inherited part of the organism, lies a problem of development," often seems elusive to behavior students. It seems unfortunate for progress along these lines that certain of our leading theorists have withdrawn somewhat from outpost positions once boldly taken. In the heredity-environment symposium, for example, Carmichael (23) discussed "The Growth of the Sensory Control of Behavior Before Birth" without reference to or use of his earlier significant theoretical contribution to this question (22), bearing on the development in utero of integrations of intrinsic and extrinsic factors influencing embryonic growth and activities. What intervening events in science have led to such changes is not clear. The significance of drug-inactivity studies with lower vertebrates (21, 164) for this question now appears somewhat equivocal. The recent study of Fromme (40) indicates that a drugged condition of inactivation has different effects upon development of aquatic locomotion in frog tadpoles, depending upon whether the drug acts prior to stage 17 or during the critical stage 17-20 interval. When these matters have been clarified, there remains the question of how far evidence from such processes on the amphibian level can be extrapolated to the level of mammalian development. Although, as Sperry (154) says, "Holt (1931) and others earlier minimized neural organization available through maturation alone," it is equally dangerous to swing to the opposite extreme for vertebrates *in general* on the basis of results suggesting the role of matured neural paths for some of the *simpler* responses in the *lower* vertebrate classes.

Currently, emphasis is surely light on the need for systematic ontogenetic behavior studies and analysis of developmental stages considered as interrelated. Analysis is usually cross sectional, and is introduced by frequently arbitrary assumptions about procedures which supposedly rule out the determinative role of extrinsic factors during early stages. For example, the method of isolation, in which a laboratory animal is raised away from others of its kind, is very often adopted as a presumably rigid control for experiential factors. However, the risk of leaning upon this crudely teleological criterion is indicated by a consideration of how the individual animal itself, as a representative of "its own kind," may be a factor through processes based upon self-

stimulative events in its own private activities. Consider a hypothesis, based on this point, with reference to the controversial problem of whether chicks recognize other chicks to any important extent on an innate basis (58, 110). Since a segregated chick usually chirps when warmed, when fed, and in similar situations, a factor of learning may be introduced favoring its approach to other chicks as against ducklings, etc., in later "choice" tests. Controlling possibilities such as this represents a challenge to the theoretical vigilance and instrumental ingenuity of the investigator.

It is clear, as Leuba (82) states, that a systematic study of innate nature is needed. This should involve analytical approaches to the problem of behavior development in a variety of animals. Another need seems to be a new methodological equipment of improved controls on the experimental intervention of preconceived ideas of innate behavior patterning. It is suggested (124) that even an insect has an ontogeny which must be considered if we are to understand its full-blown behavior pattern.

As Hall (42) reminds us, the nature-nurture question is a pseudo-argument, which has not brought science closer to understanding the contributions of heredity to behavior patterns. The field of genetics in biology has concerned itself largely with morphology, whereas psychologists have studied behavior largely in pre-Mendelian terms. The growing interest in a joint discipline of "psycho-genetics," which Hall discusses, stresses the need for a *rapprochement* of these two fields in investigation. Such studies, performed on a systematic basis, should be very useful, as one source of evidence for a systematic theoretical examination of "innate nature." But investigations of hereditary-and-behavior correlations must be paralleled or extended by analytical studies of ontogenic stages in each animal type, if evidence from such research is to be properly evaluated and validly explained in theory.

There are very different attitudes toward the value for animal psychology of studies on morphological and physiological factors underlying behavior. Skinner (31) for instance maintains that a science of behavior may be developed without reference to neurology; Loucks (88) asserts on the other hand that neurology and neurophysiology cannot be excluded. Discussing the difficulties, Lashley (76, 77) points to the fact that in some organisms where the relations are simple and direct (e.g., the earthworm) it has been possible to show correlations between specific structures and behavior, but in more complex reactions in which the influence of hereditary factors appears to be heavy (e.g., nesting in birds), clear correlative analyses have been baffled. Obviously, an even weightier challenge to understanding the role of struc-

ture in behavior is encountered in the processes of behavior modifiability. The challenge, however, is to improve theory, not to retreat with agility. How well such theory is improved may determine the relevance of evidence from psychogenetics for the problems of "innate behavior" and of ontogeny alike.

Now evolutionary theory and systematics in biology have placed a fundamental emphasis upon structural relationships among animal groups, implying some more or less important significance for adaptive adjustments in the respective groups. In the recent development of this field (65) this emphasis has been extended very actively to the consideration of behavioral factors in the adjustive repertoire of species. Animal psychologists, as Nissen (106) says, have found it difficult to see "how major psychological emergents are demonstrated to coincide with major and abrupt changes in structural characteristics differentiating larger taxonomic groups, especially the phyla." This difficulty certainly rests to a great extent in the lack of analytical comparative studies on patterns of behavior and their variations in related groups. The generalization that a direct involvement of structural factors in the functional determination of behavior patterns decreases phyletically from lower invertebrates to mammals (92) may be considered only a rather tenuous introduction to the necessary programmatic investigations.

If psychologists neglect to carry out such research systematically, others will attempt it, in their own ways. Biologists have recognized for some time that behavioral characteristics may be useful as clues to taxonomic affinities (85, 112). The heuristic value of this fact has greatly stimulated interest of biologists in animal behavior. Thus Lorenz (85, 86, 87), leader of an enthusiastic contemporary group of European zoologists devoted to studying behavior, has been led to say that:

... behavior patterns are not something which animals may do or not do, or do in different ways, according to requirements of the occasion, but something which animals of a given species have got, exactly in the same manner as they "have got" claws or teeth of a definite morphological structure. ... From the recognition of this fact it is only a very short step to the systematic comparison of the innate behavior patterns characteristic of allied species. ... [The term comparative] indicates a rather complicated method of procedures which, by studying the similarities and dissimilarities of homologous characters of allied forms, simultaneously obtains indications as to the phyletic relationships of these forms of life and as to the historical origin of the homologous characters in question (87, p. 238).

Such conclusions seem inevitable to those whose experience is bounded largely by specific behavior observations together with the realities of

morphology. There is, however, much more to this than meets the eye, and perhaps psychologists should pay more careful attention to the validity of attempts to draw up principles of behavior in direct parallel with principles of morphology.

Not just because of its practical use to taxonomists, but more important, to observe and learn its nature, Lorenz urges behavior as an important object for biologists to study. He presents them with a rather neat, positivistic theoretical system (85, 86) whereby behavior patterns are viewed as innately determined through intracentral processes in the nervous system, with specific stimulative effects acting as "releasers" to trigger them off. Lately, Lorenz (87) has begun to speak of natively determined patterns as "endogenous behavior." The investigations are essentially observational, qualitative, and relatively simple, mainly on birds although with studies on insects, fishes, and other forms also involved. The approach is avowedly comparative. However, since the theory is essentially preformistic, with an *a priori* emphasis upon the "native" aspects of behavior, a really comparative methodology of animal psychology has not yet appeared and may find difficulties in growing within this context. The proneness to impose a similar rigid nativistic ideology upon behavior in different and even widely different animals may be suggested by the final statements in a recent paper by Lorenz:

... it is high time that social and group psychology began to occupy itself with the physiological side of behavior and more especially with the innate processes of which I spoke above. It is high time that the collective human intellect got some control on the necessary outlets for certain endogenously generated drives, for instance "aggression," and some knowledge of human innate releasing mechanisms, especially those activating aggression. Hitherto it is only demagogues who seem to have a certain working knowledge of these matters and who, by devising surprisingly simple "dummies,"<sup>2</sup> are able to elicit fighting responses in human beings with about the same predictability as Tinbergen does in sticklebacks (87, pp. 266 f.).

The Lorenz system has been criticized as "too simple." With all due credit to its desirable emphasis upon behavior study, the present writer wonders about the validity of its basic postulates.

That the Lorenz conceptual system is not without its appeal to American psychologists is indicated by the numerous references to it in the recent handbook (155) in which, however, no critical appraisal is undertaken. There are indications, it must be said, that a preoccupation with a presumed native predetermination of behavior may lead to overlooking necessary controls or significant aspects of evidence. Thus from

<sup>2</sup> Models, artifacts, used in the investigation of "releasers."



an investigation frequently cited as evidence for Lorenzian principles, Tinbergen and Kuehnen (160) reported that the gaping (i.e., bill-opening) reactions of young thrushes are released initially by mechanical stimuli, but after the tenth day also by visual stimuli such as a black disc within a given size range in diameter. However, some observations by Lehrman (81) raise a serious question as to the validity of the Tinbergen-Kuehnen interpretation of the visual stage as natively determined. In the days immediately preceding that stage, the young nestling's eyes are likely to open during the latter part of the bill reaction (for example, while the parent bird is sitting on the side of the nest). A contiguity of stimuli is thereby effective with the critical response, which may introduce conditioning as a factor preparatory to the visual phase.

It is doubtful whether any methodology can be successfully comparative without a thoroughgoing concern for ascertaining the characteristics of ontogeny in each behavior system studied, as a step preliminary to the intellectual process of comparing behavior patterns and personality in different animals.

#### LEARNING

By and large, American animal psychologists have worked most intensively in the field of learning problems (52, 101, 155). Different methods have been used and differing theoretical viewpoints have arisen, represented currently by the stress of Hull's (61, 62) theory on "reinforcement processes" and by Tolman's (161, 162) on "field-cognition" processes in learning. At present, methods are more similar than previously; however, theoretical differences are still rather sharp between these two systems in particular (79, 80, 152). By and large, theoretical trends in this field are much too complex to receive any amount of treatment in the present context; hence we shall limit our discussion to some general comments which appear to be in order.

An observant bystander outside the immediate field of intensive investigation and research on learning readily gains the impression that those carrying on work in this field often are likely to grossly underestimate their own roles in the situation, as complexes of experimental variables. The crucial responsibility thus involved is indicated by Melton's (98) statement: "Stimulus response and field theorists frequently engage in the same types of investigations, with similar situations, but with such conditions imposed as to maximize the applicability of their own conceptual structure or minimize the applicability of the opponent's conceptual structure." It would seem that



among the causes of contradictory results, in addition to the variations in investigative situation and poorly known conditions mentioned by Brogden (155), the experimenter himself must play a large and frequently uncontrolled role. His responsibility for narrowing down the scope of experimentation so that a limited and "unnatural" view of the phenomenon may be obtained in the end, is suggested by Brogden's (155) reminder that "The response has rarely been the independent variable," although it may be an important variable. Aiming for strict control, an experimenter laboring under the weight of a conventionalized theory may acquire a sort of "tunnel vision" which blocks from view aspects of the phenomenon which happen not to lend themselves to convenient study under his methods, or within his attitude. Though aiming to attain a *strict* experimental control, through losing the relation of trees-focused-upon to the forest he risks the loss of validity in the study of nature. Is it possible that the contemporary field of animal-learning investigation has become overspecialized along these lines?

The conditioning and the trial-and-error approaches to the study of learning both have existed in various forms for some time, often with applications to very different contexts (52, 166). The influence of Pavlov's (111) and Thorndike's (158) findings and theory tended to be somewhat different in American psychology. Thus, after Watson's (168, 169) inspired theorizing based on conditioning theory to a great extent, some investigators emphasize contiguity and reinforcement (152); after Thorndike's versatile studies of learning, with or without the influence of McDougall (97), other investigators emphasize the initiative, goal-seeking tendency, or cognition of the organism (80). Maier and Schneirla (92, 93) distinguished between contiguity and selection as conditions making for qualitatively different processes in the learning adjustment; Skinner (142, 143) and Hilgard and Marquis (53), from an operational point of view, distinguished between classical and instrumental patterns of conditioned learning. Culler and associates (29), in particular, reported results corroborating a qualitative distinction. To Kendler and Underwood (67), from the reinforcement standpoint, the results seemed insufficient for such a distinction; however, to Birch and Bitterman (18) the evidence seemed adequate, and further evidence was cited to support the position of Maier and Schneirla (93). Reinforcement-contiguity theorists (62, 152) hold their lines to a concept of monomorphic learning without essential change. Tolman (163) has recently accepted the premise that qualitatively different learning processes exist; however, his distinguished processes *all* have

the characteristics of cognitive mechanisms. It seems quite probable that both reinforcement and cognition formulae may be reducing the phenomena of learning to what may represent a *valid* phase, but a *different* phase of the problem range.

Thus far, the principal emphasis in studying learning has been upon the highly specialized investigation of a few mammalian types, without too much emphasis upon differences in patterns. The relatively few comparative studies attempted have brought interesting results. Thus, although a spider monkey investigated by Lashley (77) could not master a similarity relationship in a matching problem, this was easy for a chimpanzee. "An attempt to analyze the learning process here results in the discovery of apparently different processes." Rats and Formica ants in suitable scale replicas of the "same" maze pattern not only learned the situation in a strikingly different manner, but adapted to postlearning changes very differently, thus evidencing different types of learned organization (126). Although available theories present no very promising resources for dealing with comparative evidence, analytical studies of learning in very different animal types should reward us with important new evidence and fresh theoretical insight.

Because of limited evidence, conclusive phyletic comparisons concerning the properties of fixation (i.e., trace formation) or of organizational capacities in learning are premature. It is difficult to see how valid comparisons can be made, outlining the scope and nature of either similarities or differences, until the learning and learning-transfer processes have been exposed in each important animal type.

With respect to the feasibility of neurophysiological postulations in the study of animal learning, very different views exist. As a rule, neither Tolman nor Hull utilizes neurophysiological concepts in his theorizing. Skinner (31, 143) maintains that a science of behavior can be built up without recourse to neural postulates. Lashley (72, 76), on the other hand, consistently has utilized neural references in his learning studies, and recently Hebb (47) has offered a "redintegrative" theory of learning and perception, grounded in concepts of neural change with experience. It is to be hoped that we may soon discover a trend toward a genuine consideration of ontogeny and personality development in learning theory.

#### INTELLIGENCE

A direct challenge to the soundness of this concept is presented by Lashley (77) when, after stating that the spider repairing a web appears

to exhibit an appreciation of spatial relationships of the same nature as concepts of spatial relationships basic to mammals, remarks that "The mechanisms of instinctive and intelligent behavior thus seem fundamentally the same." While no doubt overgenerous to the status of arthropod capacities, and although unsupported by analytical evidence, this comment nevertheless calls attention to our lack of systematic comparative studies of adaptive behavior on different levels. As Hebb (48) concludes, "We shall almost certainly find that the conception of a general level of intelligence is useless and that we shall have to learn how to define, not a single continuum, but a number of continua, requiring qualitative as well as quantitative analysis of behavior."

A stock definition of animal intelligence, "the capacity to meet new environmental conditions successfully," does not hold up well against the questions: "What conditions? How new? And how met?" To evaluate the process dependably, we must know more about the conditions of adjustment and the ontogenetic prerequisites than is usually known. Effective adjustment to an "emergency" offers an unstable criterion for intelligent behavior, for it is conceivable that different animals meet their respective crisis situations with equal degrees of success but on the basis of very different organizational processes.

"Complexity," often used as a criterion, is hardly standardized in meaning and is a weak pillar to support evaluations. As Nissen (106) states, although "the recurring term in phyletic comparisons is increasing complexity . . . [it is] of course too broad to be useful, unless the complexity is further specified." For example, in keeping with a complex morphological makeup, the starfish is capable of complex locomotive processes, in the sense of a very involved neural and stimulus-response repertoire, in which, however, a low-grade integration is predominant (43, 145). It is doubtful that complexity may be taken as evidence for intelligence status, unless the concomitant qualitative characteristics are known. Formica ants can learn complex mazes, but the habits are organized in ways inferior to those of the rat, and their application of the habit to new situations is also inferior (126). In a "molecular" sense, trial-and-error solutions by higher mammals may be more *complex* than "insight" solutions.

One great difficulty is that the composite variable to be measured and compared, intelligence capacity, is necessarily influenced to variable extents by auxiliary factors such as sensory acuity, motor dexterity, and motivation. This entire "assisting caste," as Nissen (106) terms it, must be made functionally equivalent in the compared sub-

jects, or otherwise controlled. It is of course an exceedingly difficult prospect to determine a weighting for the sensitivity factor, for instance, in animals as different as the rat and spider. For similar reasons, McBride and Hebb (96), after endeavoring to compare the intelligence of dolphins with that of higher vertebrates, reached the conclusion that at present no adequate method exists for this purpose. Not only do the auxiliary factors seem inextricably involved in the pattern, but the problem-solving process itself may be elusively different in different types of organism. These considerations suggest a need for really systematic efforts to solve the problem, not for giving up.

That results from original learning tests offer an elusive basis for comparing intelligence is indicated, for instance, by the difficulty Searle (133) experienced in trying to isolate a learning factor in analytical tests with "bright" and "dull" strains of rats. And in the "same" test situation, very different patterns of mastery may appear in initial learning (94). An extrapolation of human intelligence-test theory promises better results. A few beginnings have been made in devising batteries of tests in which, in various problem contexts, the ability to adapt a learned repertoire to new situations is measured systematically. Fischel (37, 38), in an interesting programmatic attempt to compare different animals in such terms, has tested reptiles, birds, dogs, and human subjects in a comparable situation. The subject is first permitted to master a characteristic adjustment to each of two lures of different attractiveness, then is tested in his ability to use these adjustments appropriately in a combined presentation.

Evaluating different ways of utilizing past experience is of course a most difficult assignment, as the history of the multiple-choice test has shown (150, 184). Using the discrimination-generalization test (cf. 114) in a programmatic way, Harlow (44) and his collaborators have outlined the manner in which a monkey advances by stages of experience in the situation through successive "learning sets," from simple to complex and plastic reaction modes. Harlow criticizes the S-R theorists for their failure to investigate adequately the experience variable in its contributions to new situational adjustments. In most learning experiments, insufficient attention has been paid to this variable (1, 25, 92). Preparation is of course critical (66, 109), since experimenters working on both the animal (17) and the human levels (33) have obtained trial-and-error or insight solutions, depending upon the manner in which part-processes were contributed through controlled preliminary experience.

There is discernible in the literature a disposition to question the



existence of an important qualitative difference between trial-and-error and insight solutions. Although this attitude is quite proper, and in accordance with Morgan's "canon," it may encourage too narrow a view in studying this crucial question. For instance, from his discrimination-generalization experiments, Harlow (44) is inclined to believe that insight is but a further stage of trial-and-error process in sequential learning, and not really qualitatively different from it. Prior to these Wisconsin experiments, however, Maier and Schneirla (92) questioned the adequacy of abstraction-generalization methods *by themselves* for differentiating "higher processes," on grounds which have not been controverted. These processes are undeniably related, but possible qualitative differences between them are not necessarily brought out by all types of test situation. The Wolfe and Spragg (178) investigation, designed to find whether Maier's (89) evidence for "combination of isolated experiences" could be reduced to the strict terms of contiguity-learning theory, did not succeed in this respect (90). In an answer to G. Allport's (7) challenge for clear evidence of higher processes in infrahuman animals, Seward (134) mustered strict criteria for insightful solutions which he found were met by Maier's recombination tests as well as by five other methods used to investigate such capacities in lower mammals.

Methodological and theoretical vigilance is of course essential to guard against the postulation of exaggerated solution capacities. Unfortunately, Morgan's canon is not accorded a basic place with equal seriousness by all students of animal behavior. Thus, in evaluating the remarkably manlike solutions achieved by monkeys in the Liège investigations, Lashley (74) concluded that the results were so influenced by uncontrolled variables (e.g., experimenter-derived cues) as to be without value for appraising the level of the solution processes involved (see also 56). As another example, a sudden fall in a learning curve may indicate a solution process of high or low qualitative status; the shape of the curve is not a critical indication in itself, but the judgment must depend upon a detailed analysis of the experimental results (92).

Often the status of an observed situational adjustment seems to depend upon a sweeping characterization rather than upon a critical study of how the situational properties are met. Thus if maze learning is defined broadly along the lines of hormic theory as "goal response," then roundabout solutions may be attributed to all animals, from the earthworm to man, which have been able to learn some kind of maze (15). On the other hand, the concept of "Umweg" may acquire value



for solution comparisons among widely different animals provided that tests and evaluation of results are guided by strict criteria (38). Yet again, the criteria of Umweg-solution are called into question by experiments such as those of Schiller (120, 121), from which roundabout solutions are reported for various inframammalian animals including cephalopods and minnows. In the same sense, the present criteria and meaning of "delayed-response" adjustments are really not adequate to appraise some reports of Thorpe (159), who describes cases in which solitary wasps maintained a route despite forced lateral deviations, and of Baerends (10), who cites the return of observed solitary wasps after intervals of time to provision appropriately each of two or three different burrows. Such developments remind us that some years ago the adequacy of the delayed-response method for calibrating higher adjustive capacities was challenged (92). One major difficulty is in controlling the extent to which critical cues are supplied by the animal or by the environmental situation. Far too little attention is being accorded to the question of evaluative criteria in discussing such problems. This condition seems not unrelated to a strong current tendency to merge superficially similar kinds of performance in very different animal types under the same phenomenal headings. Unclear comparisons with poorly devised concepts are considerably worse than no comparisons at all.

#### PERCEPTION

The application of this term to broadly different types of adjustment has increased to such an extent that clarification is urgently needed. On the one hand, experimental investigations have been directed at the analysis of perceptual phenomena (47, 73), and the continuity-discontinuity controversy (75, 153) is another expression of efforts to study the nature of situational meaning and organization in perceptual discriminations. On the other hand, use of the term on a vague qualitative basis in the general literature has increased rather than decreased.

There exists a widespread tendency to employ the word "perception" rather loosely to any case in which an animal senses an object or situation and reacts to it in a manner considered "adaptive" (e.g., 119, 156). Whether the response is to be considered a reaction simply and directly elicited on an uncomplicated sensory-motor basis, or an appropriate reaction to the pattern of a recognized familiar object or situation, is very often very unclear. In the absence of specific evaluative criteria or even of a disposition to be critical in the matter, if

such practices continue, the scientific role of this term in any conceptual sense may be reduced to that of a minus factor. Thus it is difficult to trace the specific implications of a statement such as Hartley's (156) for certain birds, that "... recognition of some enemies results from innate powers." Certain stimulative and behavioral relationships are described or implied in such cases, but the unknown status of the adjustment would seem to merit a more operational and less presumptive characterization.

Scattered experimental developments suggest that it is important to be vigilant for differences in the sensory-integrative processes underlying adjustments to situations by animals. For example, Hertz (50, 51) investigated the visual adjustments of bees to a variety of artifacts, and formulated a rather complex theory to account for the bee's ability to single out from the first those with greater brokenness of outline or greater internal complexity. Particular training was not needed; in fact, such response tendencies strongly opposed any training of positive reactions to the opposite visual condition. The investigations of Wolf (176) disclosed that such visually determined reactions in the bee are attributable to differences in flicker effect, and not to a visual pattern organization in the human sense so far as was discernible.

Unfortunately, the problem of "innate" contributions to perception has not been attacked very systematically and analytically on any subhuman level. The nature of differences between "those animals whose repertoire of perceptions is more or less limited to the relatively small number determined by innate organization . . . [and] those animals whose perceptual organizations are primarily acquired . . .," as Nissen (106) suggests the distinction, is very poorly understood. What physical characteristics such as intensity, rate of movement, size, and so on, may produce adaptive reactions in animals without benefit of specific experience, is a subject that must be approached through methods appropriate to the animal type concerned. It is encouraging to note a disposition on the part of some psychologists to meet such problems frontally. Hebb (47), for one, has endeavored to develop a theory of perceptual development on the mammalian level, in the light of both native factors and of ontogenetic processes.

At the same time, less inductive approaches to these problems are in evidence. It is with problems concerning sensory apprehension and adjustment that Lorenz and his associates (156) have been especially involved. They have observed and classified for a considerable number of animals, particularly birds but also fishes and certain other lower vertebrates, the types of stimulus-effect ("releasers") which will pro-

duce given species-characteristic reactions. These reactions, as mentioned before, are considered innate and intraneurally determined, each with its corresponding extrinsic releaser, or trigger stimulus. The execution of the releasing-stimulus activity represents the end or goal toward which appetitive behavior is directed (85, 87). The releasing stimulus (156) is typically named according to the observer's appraisal of the animal's (adaptive) behavioral relation to the situation of occurrence (e.g., an "enemy" valence). No particular differences are set up by these investigators between the postulated "innately given" qualities of objects or situations and the perceptual characteristics of learned, familiar situations in which the character of adjustment depends upon experience (47, 81). Usually the releaser or the "sign stimulus" is described rather teleologically by the observer, whether it depends upon those features "most typical for the object" or "a configuration of features" (11). Some psychologists, troubled by the characteristic employment of observer-subjective naming devices instead of critically selected operational terms, may wonder as to our safeguards for reliability in weighting "innate" and "experience" variables, and for presuming to distinguish them in theory, when investigative methods are really cross sectional rather than ontogenetic. In view of our lack of adequate criteria for controlling such factors, the assurance of the outspoken Lorenz group is amazing.

Clarification is surely needed for the problem of "innately given" sensory adjustments to objects and situations, and their relation to the contribution of learning. How far are qualitative aspects (i.e., "meaning character") of the situation presumed to differ for the animal in innately controlled cases as against instances in which learning intervenes? To what extent and how does a pigeon "perceive *her young*," when upon their removal to a short distance she returns to sit upon the nest, while they shiver in full view? In preliminary investigations, Riess (118) found that female rats, after they had passed their early postweaning life in isolation without ever having handled or carried objects of any kind (subsisting entirely upon powdered food), in the parturition situation were very inferior in all respects to normal mothers, and lost all of their young promptly. What was the deficiency of these rats in early experience, as compared with normal rats? The hypothesis that the absent factor here is a perception of objects-to-be-approached-and-dealt-with (i.e., as "incentives") seems to merit testing. It appears that without the lacking factor a "maternal behavior pattern" cannot be formed.

The importance of experience for the development of visual per-

ceptual adjustments in chimpanzees is indicated by Riesen's (116) investigation with chimpanzees raised in darkness from birth to 16 months of age. Since, in tests then carried out, objects evidently had no more meaning for these animals than for newborn chimpanzees, questions arise concerning the normal ontogeny of perception. (The possible contribution of previous perceptual training through other sensory modalities—e.g., somesthetic—as factors in re-education through intersensory transfer (78) will bear further consideration.) Such experiments indicate the development of a renewed interest in the ontogeny of individual perception in lower animals.

Investigations of the development of external drive controls in motivation contribute to an understanding of perceptual development. An early contribution was Woodworth's (180) concept of habits becoming transformed into drives through experience; a recent development has been the concept of "externalization of drives" (9). A predominant assumption is that motivational growth in mammals involves the specialization of the drive-behavior pattern with respect to particular learned situational adjustments. The characteristics of objects and situations which are connected with the relief of needs thus become focalized in perception, in a manner which much research on discriminative learning has been directed toward clarifying. The relationship of such processes to meeting the conditions of the environment is strikingly represented in the more rapid and effective acquisition of token-reward habit by chimpanzees (177) than by the cat (147), and its more plastic use by the former animal.

In the theory projected by Hebb (47) to account for ontogenetic perceptual development, the growth of perceptual patterns is traced for a mammalian animal from the naive stage through the first stages of learning. A developing action basis of expectancy and attention, a simple and molar one in this case, is followed through in an inductive procedure. This theory, which is characterized as a "redintegrative" developmental theory, appears to furnish a sound approach and good basis for further ontogenetic theory in child and animal psychology. Perception at any stage is regarded as involving an expectancy which is selective in function, and which thereby influences situational adjustments and further learning. What is learned at any stage depends on what can then be perceived, i.e., it is a function of how far the perceptual systems have advanced.

An inductive theory of perceptual development such as this calls attention to the problem of set or expectancy in situational adjustment and learning. It contains a further potentiality: namely, that relatively

limited sets or adjustive patterns may represent early stages of more dynamic and versatile systems of expectancy, according to the animal's capacities for such advances. Is a conceptual clarification developing through such considerations—in that the term "set" may be applied more specifically to a functionally earlier, more limited (in selective scope) and stereotyped adjustive pattern? It is possible that, in contrast, a concept such as Maier's (91) "direction" represents the more dynamic expectancies of a later stage, through which the organism may overcome the bonds of specific contiguities more readily than before.

A groping for the relationship between perceptual and conceptual processes is currently apparent. It seems possible that perceptual systems, under appropriate conditions, may lead to conceptual systems. One interesting experimental development may be subject to reinterpretation in this light. After the Hayeses (46) had raised an infant chimpanzee under home conditions as closely similar as possible to those normal for a human infant, they reached the conclusion that this infant was inferior in no identifiable way to a human infant in its course of development until the language stage was reached in the latter. The principal psychological deficiency of chimpanzee ontogeny thus is attributed to the absence of a verbal language. On the other hand, it seems possible that preliminary stages of perceptual development, difficult to follow in observational studies, are partially similar in man and chimpanzee but also different in subtle qualitative ways. The hypothesis that the infant chimpanzee never reaches the threshold of perceptual-meaning-organization and nonlinguistic conception which is prerequisite for verbal-symbol mastery demands testing.

What is the relationship between perception and conceptual processes in mammalian ontogeny? Some writers seek an answer along the path of a "lumping" procedure. Thus Leeper (80) insists that the principal characteristics of "inductive concept formation are also to be found in discrimination learning and conditioning, and much of trial-and-error learning." These characteristics are given as differentiation and some generality of application. Whether this is to be considered a speculative exercise, or an introduction to a serious systematic reductive operation on the part of cognitive theory, is not altogether clear. It is often forgotten that in such exercises there is also a responsibility for trying to discern the significance of qualitative differences in the compared processes and capacities. It seems worth suggesting that a greater theoretical gain may lie in regarding these processes as successive stages in the development of an individual, i.e., in viewing conditioning



as a process prerequisite to the more complex organizations of trial-and-error or selective learning, perception as a prerequisite to more highly involved functions of conceptual mastery, and the like. What is needed, perhaps, is a closer application of the comparative method to the tracing of stages in the development of personality on different animal levels.

#### A COMPARATIVE SOCIAL PSYCHOLOGY OF ANIMALS

Animal social investigations employing the comparative method are still uncommon, although there are indications of new developments both in research and in theory (4, 125). Psychologists have been less active here than biologists (26, 132). There are, however, a few indications of convergent developments in biology, psychology, and sociology (3, 131), pointing to further systematic investigation of social phenomena in animals. Advances are needed particularly in studying what differences may be significant for differentiating various types of animal societies, while at the same time their similarities are taken into account.

It is probable that the study of animal social behavior has resources which have not been brought to the investigative level attained by the study of human group dynamics (4, 126). Although much of the animal work is still descriptive, there are some detailed and systematic studies with implications for further research (24, 30, 57, 123).

The general conceptual trend is still influenced by intergroup similarities, much less by contrasts. An example is the concept of "dominance hierarchies," which has appealed to many laboratory investigators, including psychologists (26, 95, 104, 122). The "peck-right" concept has furnished a convenient way of describing the group behavior of vertebrate animals, and dominance-submission relations certainly have proved to be an outstanding aspect of group relations, as studied both in laboratory and field. It is now apparent, however, that this concept is somewhat like that of "extroversion-introversion" in the sense that it represents not just one social parameter such as aggression relations (26, 95), but a number of variables such as ascendance (186), prestige, and the like. Its main limitations may be summed up by saying that the dominance concept represents only one aspect of group behavior, which does not necessarily reveal the factors essential to group structure or may even reveal them in a negative way (125, 129).

The "dominance-hierarchy" concept, moreover, has influenced the attention and thought of students at the expense of concepts relating

to the more positive and central aspects of group structure, so that the potentialities of concepts such as "cooperation" (3, 28, 41, 125, 185) seem largely unrealized. Although it is true of course that, as Zuckerman (188) has said, a comparative social psychology cannot exclude reference to the principle of dominance, it is now clear that unless dominance behavior is viewed in its relation to other social factors, investigation and theory in this field are certain to be seriously limited and distorted (3, 125).

The usefulness of other concepts such as "cooperation" has been reduced greatly by tendencies to use them in a phyletically generalized way. Thus questions concerning the rise of gregariousness and the properties of group organization in different animal types have been approached only to a limited extent in research and theory. Investigations inspired by "innate" behavior theory (156) emphasize a descriptive type of study, concentrate on individual aspects of social behavior, and do not attack the problems of group structure in analytical ways. It should be noted that questions of group communicative behavior have been attacked on several levels (14, 39, 107, 115, 125, 187), although mainly only in preliminary ways. However, an adequately comparative program of study of such phenomena is long overdue, particularly to clarify the relationships of concepts such as "sign," "signal," and "symbol," as well as the criteria of "language" (14, 115), all of which appear to suffer from a heavy load of speculation and a minimum of systematic research. Research on questions concerning levels of phenotypic relationships through successive generations in lower animal groups, certainly basic to a needed re-evaluation of the broad problem of "culture" (4, 125, 137), should enlist the active attention of social psychologists, sociologists, and anthropologists alike.

It would seem that a sufficient number of objective studies of group behavior have been carried out (24, 28, 30, 57, 123, 132) on widely different types of animals and groups, with prospects of others in a more intensive vein, to justify a new and more concentrated attack upon theoretical problems common to all group organizations. The basic biological aspects of such phenomena have been investigated theoretically by Allee (3, 5) in particular. The central idea for a consistent theoretical statement as to the properties of "gregariousness" on different levels seems to be available in the potentialities of Wheeler's (172) concept of "trophallaxis" (i.e., food-exchange relationships). This concept has potentialities for a much broader extension in physiological psychology as a means of appraising the variant social impacts of funda-

mental biological processes in the growth and maintenance of different types of animal groups. A preliminary conceptualization of widely different types of group patterns, e.g., insect and higher mammalian structures, on this basis, has been offered by Schneirla (125, 129) as the "bio-social" and the "psycho-social."

A limited but promising vigorous trend toward the study of animal group behavior has been apparent, and an inevitable enlargement of interest in the questions of the relationship of familial functions to the origin and continuance of larger groups is to be expected. There are a few beginnings in research on individual socialization in different animal types. A flourishing positivistic conceptualization of individual and group relationships in evolution, with the group characterized as "supraorganism" (5, 6, 34, 35), has been found by some biologists to have heuristic value. On the other hand, it has been criticized (108, 125) for having weaknesses common to general analogies, which in this case readily lead to serious misrepresentations of individual and group characteristics, and do not suggest or encourage comparative analysis.

#### ASPECTS OF METHOD AND ATTITUDE

Attitudes and practices concerning method are of course critical for the possibility of a genuine comparative basis for animal psychology. A few important points may be considered here. There appears to be a growing awareness that in the vast experimental literature on rat behavior, really very little psychological information about the pre-experimental antecedents and life of the experimental subjects is given as a rule. The subjects are usually regarded as "naive" and "inexperienced" if they have had no *specific* experience, to the experimenter's knowledge, in the *specific* kind of situation he is investigating. Of course, the literature for some time has not lacked reminders, and often strong reminders (1, 25, 48, 63, 66, 109, 117), of the fact that perceptual, emotional, motivational, and other influential aspects of the rat's personality must be considered and controlled as important experimental variables. Christie (25) calls attention to the probability that "these variations might have an appreciable effect upon the behavior of adult rats and might underlie some of the contradictory findings reported in the literature." Of this, it would seem, we can be positive.

This fact calls attention to the attitudes and predilections of the investigator himself as a most important variable in research on animal behavior. The reasons for a common lack of attention to the experience factor, and a general disposition to assume it to be homogeneous in the group and thus controlled, unfortunately cannot be considered simple

and readily correctible. The symptoms center about a strong tendency to focus upon a specific problem without any adequate consideration of its broader naturalistic implications. There is in general too little interest in the ontogeny of behavior and personality development as important problems demanding investigation. Mainly for this reason, no doubt, a myopia is detectable for experiential factors which may influence behavior phenomena in somewhat obscure and indirect ways, and for their possible equivalence in function to "training" designedly introduced by the experimenter. As an example, Hudson (59) points out that:

Observation of the behavior of an animal during the course of a typical conditioning experiment will often suggest that important aspects of the learning process are not available for scrutiny by that method. Resistance of the animal to being placed in the apparatus, for example, may develop long before the appearance of a conditioned motor response. This, as a relevant aspect of the learning, is sometimes overlooked.

Hudson emphasizes the significance of rapidly learned and very important perceptual reactions for the behavior of subjects in test situations. Cage life contributes psychological factors of this sort, however difficult they may be to identify. This point deserves renewed emphasis, to counteract the implicit assumption that, if subjects are "naive" for the *specific* problem, "training" then becomes a function of the experimenter's specific operations. This limitation may defeat the efforts of the most hard-working investigator for "rigid control."

As these considerations suggest, much contemporary work in animal psychology tends toward a segmental approach to the organism, through an intense concentration upon specific problems without a full appreciation of their behavioral setting. Thus, rigid control of a small-scale situation in the study of learning may mean a poor control in regard to the broad problem of personality development. A heavy burden is placed on the experimenter, proceeding operationally. Through his own subjective impressions and selective processes in evaluating the phenomenon, he decides what independent variables he shall measure and what "dependent variables" he shall control—devising situation, apparatus, and procedure accordingly. An experimenter, an S-R experimenter in particular, thus may not realize what he excludes when he selects for the animal what the animal is to learn. Planned along the line of "rigid" thought processes dictated by a particular theoretical structure, the experimenter's preconception of the phenomenon may be so imposed upon the investigative process that it becomes not a *test* of anything so much as it is a *demonstration* of that

theory. As Harlow (44) has pointed out, S-R theorists do not really control the experience variable in learning studies, notwithstanding their stress upon a "historical" approach to learning. A similar error is made at times by cognitive psychologists in postulating systematic solution tendencies and insight in the animal's approach to a situation (69, 70) without adequate attention to simpler alternative factors (175).

These are certainly not arguments against control in research on behavior problems; on the contrary, they suggest the need for basic *improvements* in control. Pre-experimental anecdotalists attributed higher mental processes to lower animals, or denied such processes, largely on the basis of stories. Their experimentally-minded successors, objectively oriented, may themselves indulge in a similar procedure more subtly derived from uncontrolled implicit processes in the experimenter as thinker and perceiver.

The expression *molar approach* is often used when the approach really is segmental. Research may be concentrated upon taking kinds of data which are partial, and thus may favor a really spurious answer to the problem. In the vast and devious literature on maze-learning in rats, how often would obscure results be clearer in their implications if the investigator had reported his data in terms of "kinds of errors" instead of (or in addition to) "error totals"? The experimenter's sagacity determines *what is significant* when he plans the investigation. He may be or feel under pressure to produce results, and thus may attempt a specialized study prematurely. As Sears (31) puts the matter, in appealing for really "molar" investigations in child study: "A kymograph cannot catch what is the essence of the child's conduct, whereas a human observer can." This is not a light or obvious point, as an uninformed bystander might think.

No argument is intended against quantitative methods in animal study, when the appropriate stage has been reached. The neglect of instrumental approaches at the required points in research may be serious for the validity of theory, as is suggested by Köhler's (68) statement that "...enthusiasm is no safe substitute for a high reliability of differences."

The compulsive advance of some psychologists toward phenomenalism, a development for which Tolman deserves a good measure of credit (151), must be recognized at least in part as one contemporary reaction to operationism, rigid use of quantitative methods, and deductivism in studying animal behavior. There has resulted an emphasis upon a strictly individualistic qualitative study of behavior which is often a



poor substitute for a frank, pre-experimental "mentalistic" study of animals.<sup>3</sup> Such an approach may effectively exclude a consideration of behavior in a broadly naturalistic and social context, in the way that Sherif (136, 137) and Hebb (47), for example, undertake to study behavior.

An emphasis upon *similarities* in process and capacity is predominant in current animal psychology. For example, does the loose habit of applying the term "learning" to instances of temporarily changed behavior in lower invertebrate animals imply some essential equivalence to "learning" in mammals? Such phenomena in the low invertebrates exhibit features much like those of sensory adaptation and muscular fatigue in higher animals, occurrences not ordinarily spoken of as "learning." The terms perception and learning are endowed a priori with some vague essence of equivalence wherever used, or the characteristics of "cognition" are found generalizable throughout a range of processes from conditioning to reasoning. In effect, neither S-R nor cognitive theories have as yet shown their worth to differentiate the various kinds of adjustive and organizing process through the animal series, although some attempts have been made (60, 61, 62, 161, 162).

The concept "purpose," long a center of controversy in biology (2, 32, 83) and in psychology (31, 55, 97), has been receiving some study in behavior work. To this trend not only the behavioristic criticisms of Watson (168) but the more eclectic "purposive behaviorism" of Tolman (161) has contributed. Previously offered as representing impulsions toward goal or end situations through the entire scale of organisms, this concept is now more clearly seen as a confusion of "adaptive behavior" with a vaguely conceived mental process (55, 85, 92, 129). The term "purpose" itself is held to require the satisfaction of certain operationally definable criteria which thus far have validated the postulation of "anticipation," "expectancy," "goal-gradient," and similar higher-level processes only in higher mammals.

A clearer understanding of the relationship of quantitative methods to observational and essentially qualitative methods is very desirable. It may be contended that there need be no fundamental dissimilarity between observation and formal experimentation, considered as scientific disciplines (36, 130). To be sure, an important part of every behavior student's training should be learning how to recognize, and dis-

<sup>3</sup> It is interesting to note that, for different explicit reasons, S-R theorists (61, 151) and phenomenalist theorists (148, 149) are inclined to exclude physiological evidence more or less completely. Notwithstanding this trend, physiological psychology continues to be a flourishing field of investigation (155).

count, an observational report which is merely anecdotal and hence unreliable. By the same token, he should also learn the clues to unreliability in experimental reports. "Hit-and-run" studies are found in both areas. Observational study, if performed systematically with an adequate methodology, can have the advantages of experimental control held as standard in laboratory work. Furthermore, a systematic observational investigation, through progressive stages of test and control, must inevitably lead into more formally devised laboratory experiments as an indispensable part of the program.

These considerations suggest the advantages of correlative studies of animal behavior adjustments under natural and under man-devised (i.e., laboratory) conditions. A striking illustration is offered in the work of Hediger (49), who has compared with insight the adaptive behavior of the same animal types in the wild and under conditions of confinement in zoos. In general, however, animal psychology has been very deficient in attention to its naturalistic aspects. We should plan to learn how various types of animals may utilize their capacities (e. g., learning) in meeting the typical hazards and arduous trials of their habitats. By remedying this lack, we should gain a more adequate appreciation of the important types of animals as integrated, functioning organisms, and should be in a better position to select validly what is important for laboratory study.

#### THE DOCTRINE OF LEVELS

A concept of "levels" in capacities and organization follows inevitably from the theory of evolution. According to the levels doctrine, existing systems, both organic and inorganic, are viewed as comparable in their structural and functional characteristics (105, 108, 125, 129), and as capable of being ranked with respect to their degrees and kinds of similarities and differences in these respects. Such a concept offers the broad framework for a systematic study of phyletic similarities and differences which is urgently needed by animal psychology if it is to develop a comparative methodology.

One immediate gain from this viewpoint is vigilant attention to the validity of extrapolations from one system to another. How far, for example, may mechanical models be used in the study of organic and behavioral systems (167, 173)? The gains to be realized through a reference from mechanical calculating devices to the function of a mammalian brain may be elusive, since rough similarities may be stressed and crucial differences (e.g., the role of the human planner and operator of the machine) not really taken into account. As another example,

evidence from the study of spinal-reflex systems has been extrapolated to the level of brain function, thereby imposing the pattern of a lower-level system upon a higher functional level in the organism. The investigations of animal psychologists, and of Lashley (72, 76) in particular, have gone far toward freeing psychology from theoretical error along this line.

A contemporary example of a reductionistic (144) theory is that of Lorenz (85, 87). This theory bases postulations concerning the innate control of general behavior patterns upon results such as v. Holst's (156), obtained from the study of *lower-level* neurophysiological functions in *lower-vertebrate* locomotion. The extrapolation here is from one type of pattern, locomotion (which might be considered a lower-level function in the individual), to organizations concerning the *whole* individual in action. Although it is a secondary matter here, the validity of the conclusions deduced from the lower-level function itself (i.e., of an endogenous, intraneural control of *patterns*) has been called into question (84, 170). Studies on the deficiencies of conditioned-response learning patterns in decorticate mammals (139) and the deficiencies of a reeducative recovery of locomotor control in spinal mammals as compared with intact individuals (140) serve to emphasize the qualitative differences of lower and higher part-processes in the organism.

The concept of levels has frequently come into discussion in the biological literature (105, 108, 113), less frequently in psychology (135, 182). Discussion of its potentialities for behavioral comparisons have been only introductory (125, 129) and occasional. The idea certainly is not a novel one with reference to behavioral capacities, since Aristotle described vegetative, sensitive, and rational souls for plants, animals, and man respectively, and both C. Lloyd Morgan (99) and Yerkes (182) utilized the essential principle of functional levels.

Certain generalizations may be derived from a preliminary comparative consideration of behavioral capacities in the principal types of living animals (92, 129). First of all, the relevance of evidence concerning morphological equipment may be considerably different according to the type of organism studied. Simply stated, in organisms considered lowest in a psychological sense, organic structure is found to have a rather immediate or "directly determining" efficacy for behavior, relatively unlike its significance for the pattern of behavior in higher organisms, in which varying stages of development and intervening part-processes must be considered. A second generalization is that any functional part-process (e.g., a sensitivity factor) may have

different functional significance on different behavioral levels, according to the nature of the whole system within which it functions. Thus, on one level, knowledge about a particular functional component may be expected to have a different bearing upon whole-individual organization from that on other levels. This generalization sharply challenges the validity of extrapolations from level to level when the comparison is made in terms of similarities alone. In other words, extrapolations from level to level concerning similar part-processes have no necessary validity. If recapitulation doctrine has its limitations for modern biology (54), it has far less intrinsic validity for the theoretical psychologist.<sup>4</sup>

Consequently, the principles of behavior must be worked out carefully on an inductive basis for each important type of animal, as a foundation for carrying forward a valid system of interlevel comparisons. Comparisons based on similarities alone may be worthless, however striking they may be. Also, behavioral complexity as such offers no dependable basis for interlevel extrapolations, since complexity is found both in qualitatively high and in qualitatively low functional levels. The clear inference is that unless animal psychologists accept the task of making thoroughgoing functional studies of the principal and widely different types of living organisms, their generalizations will have validity only for the levels studied.

It is evident that knowledge and theory concerning phyletic positions and evolutionary background, as determined on biological grounds, must be treated as challenging and introductory by the animal psychologist, for whom the essential question concerns the degree of phyletic relationship or the lack of such relationships. For the psychologist, working contemporaneously, there remains the task of determining the significance of such evidence for the interpretation of behavioral capacities and organization in each type of living organism. Biologists consider existing animal forms as an incomplete series from which transitional forms have largely disappeared through natural selection (141). Psychologists must therefore study animal types as they find them, and rank them accordingly.

The levels viewpoint also emphasizes the dangers inherent in the teleological practice of naming behavior functions according to similar adaptive outcomes, such as food-getting, or protective behavior. Behavioral similarities alone, and similar adaptive results, offer deceptive clues to underlying processes in different animal forms. In terms of the levels concept, we become alert not only for evidence warranting

<sup>4</sup> A limited heuristic value may be granted to Haeckel's law as applied to behavior, but no assured *a priori* place in theory.

the postulation of qualitative similarities, but also for qualitative differences at crucial turning points in the phyletic scale. A tendency to rely closely upon evidence concerning morphological turning points thus becomes educated through the consideration of behavioral qualifications. It is to be expected that difficulties will be experienced in relating such behavioral qualifications to structural turning points (76, 77), for at progressively higher psychological levels the relationship of specific structural equipment to behavior becomes increasingly devious.

Consequently, the meaning of the terms "part-process" (the molecular) and of more inclusive organization (the molar) must be worked out in a study of behavioral development and organization on each functional level. Then, by contrasting various levels, light may be thrown upon common problems in the field, such as learning. Premature comparisons, ruled by a preoccupation with similarities alone, cannot have much validity for a scientific, hence comparative, psychological theory.

Without doubt, a comparative study of behavioral development and capacities on different animal levels should stimulate investigations on the development of human capacities and personality, in ways perhaps otherwise unrealized. The answer to challenges of the relevance of animal-behavior evidence for human psychology is not that, in the absence of such evidence, irresponsible conclusions will be drawn and applied in any case, both in lay and in scientific circles. More significant is the greater breadth of view which can be obtained through comparative methods in studying the wide range of psychological problems.

This discussion may end with its initial statement concerning a defensible ideal in comparative psychology. One reason for encouraging such a discipline is to gain a valid grasp of man's similarities and differences with respect to lower animals, to replace the often irresponsible speculation which otherwise flourishes in general theory. A wider ideal is that the effective use of the comparative methodology of science in the basic fields of animal, child, and social study is certain to contribute to the conceptual development of all of these fields and thus benefit psychology as a whole.

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THE SOCIAL BEHAVIOR OF VERTEBRATES: A REVIEW  
OF THE LITERATURE (1939-1950)

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An experimental approach to some of the biosocial problems of human social interaction is frequently made through studies on lower animal forms. Beach (28) states that, "We are just beginning to appreciate the usefulness of a comparative study of social behavior. The findings . . . point to the potential advantage of using a variety of animal species in our investigations of interactions between members of a social group" (p. 123). Furthermore, he stated that unless experimentalists escaped the spell that *Rattus Norvegicus* was casting over them, even the experimentalists themselves were in danger of extinction (p. 117). Beach reached this conclusion from an analysis of the studies reported in all odd-numbered volumes of the *Journal of Animal Behavior* and the *Journal of Comparative and Physiological Psychology*. Beach's thesis in this paper is that a comparative study of social behavior based on a variety of species would be a most valuable scientific enterprise.

That there has been a dearth of studies of social behavior of animals might seem to be the case if one referred to the chapter by Nissen (168) in the recently published *Handbook of Experimental Psychology*. On the other hand, reference to chapters by Nissen (169) and Collias (72) in Stone's *Comparative Psychology* would indicate that the study of social behavior of animals has not vanished. The reported studies, however, are scattered in many journals, and most certainly are not (in full agreement with Beach's analysis) concentrated in the main in psychological journals.

The purpose of this paper is to bring together the scattered literature in review form so that ready reference to the rather extensive literature is available to the reader. Crawford (77) has reviewed the literature to 1939, and the present writers have covered the period from 1939 to 1950, following the general organization used by Crawford.

There are a number of references covering a wider range of topics than will be considered here, as well as several references which are reviews of particular aspects of social behavior. Allee *et al.* (16) have summarized much of the literature on animal ecology, and this text is of value for the chapter on animal aggregations. The principles of cooperation and disoperation are proposed as explanatory concepts of

sociality. This general view of Allee's is presented in other, more limited discussions (4, 5, 7, 8, 9,) which emphasize the survival values of social behavior. Other reviews and discussions of social behavior may be found in Tinklepaugh (234), Darling (87), Lack (146), Harding (128), Faure (98), Walker (239), Cattell (64), Redfield (178), and Bruce (52).

An excellent monograph on the methodology for studying animal societies has been edited by Scott (212). The papers by Carpenter (63) and Schneirla (193) are useful for those who are interested in applying objective methods to the study of behavior under field conditions. These two writers present a very convincing discussion of the applicability of techniques customarily associated with the laboratory to animal groups living in their natural habitat. Carpenter's success in this area (57, 58, 60) is well known. Scott's (208) monograph on sheep and his publications on mice (205), goats (206), and dogs and wolves (213, 214) represent a more limited application of objective methods to the study of social behavior in "natural" groups. Scott (216) has also been successful in gathering significant data on the social behavior of three little known species of grouse. Reported studies of various groups of vertebrates living in seminatural habitats in zoological parks indicate the value to a comparative social psychology of studying a range of species. For summaries of this work, see Collias (69, 70, 71, 72). Consideration of other methodological problems is given in Fuhr (107) and Hebb (133, 134).

Haskell (129) has presented criteria for a natural classification of all societies, including that of man. The classification is determined, in part, by a property called "coaction," a characteristic of a situation in which the "stronger individuals either increase or decrease this (life) process' normal rate in weaker ones, or vice versa, or both" (p. 187). In a related discussion, Essen (97) analyzes the interpersonal behavior of animals and concludes that the interactive evaluation is primarily dependent upon physiological and learning factors. Following this general line of a quantitative evaluation of interaction, Dice (89) suggests the use of Forbes's coefficient of association to provide a measure of the amount of association of one species with another.

#### AGGREGATIONAL BEHAVIOR

The best source of information on aggregations or "contagious distributions" may be found in the work of Allee and his collaborators (3, 4, 5, 8, 10, 16). The various factors responsible for aggregations and one theoretical explanation of them can be found in Allee *et al.* (16).

Other theoretical discussions of aggregation have been presented by Fraenkel and Gunn (103), and Rabaud (176).

*Fish.* Practically all of the experimental work in the past decade on the topic of aggregational behavior in vertebrates seems to have been limited to fish. Allee *et al.* (15) have found a favorable effect on the growth of goldfish which are raised in water in which other individuals of the same species have resided. This effect would be predicted by the principle of coaction (129), which has been supported primarily by the findings of Langlois (147) on bass. Schlaifer (223, 224) has reported that the goldfish has a lower rate of locomotor activity and oxygen consumption when grouped than when isolated. This "group effect" is said to be dependent on visual responses to other goldfish or "goldfish-like forms." Breder and Nigrelli (45) have confirmed an earlier finding that the swimming speed or locomotor activity of fishes is related to the size of the aquarium and the number of fish present. Data were obtained in each of three dimensions by ruling coordinates on the glass walls. Breder (44) discusses measures of quantifying the degree of the aggregating tendency. By using models of various sizes and live fish of matching colors, several interesting results were obtained. Targets of a particular size led to approach, but if a larger target was presented, retreat occurred. Groups of like color seemed to be more cohesive than groups containing fish unlike in color. Isolation from the group for several months modified the aggregating pattern and the isolates were sensitive to previously ineffective stimulation.

Schlaifer and Breder (225) report a tendency for tarpon to rise to breathe when other individuals rise. Using models, they were able to induce "imitative" rises in tarpon. These rises are said to occur only when there is a depletion of oxygen in the swim bladder. In a later study, Breder (43) reports no increase in the number of rises with an increase in temperature as had been found in small tarpon. Furthermore, nearly every rise in the mature tarpon was individual and not imitative as in the small fish.

Size plays an important part in the isolation of minnows from a well-habituated minnow shoal according to Berwein (35). Minnows smaller or larger than the shoal by approximately twelve millimeters are isolates. This isolation is not affected by temperature or extirpation of the cerebrum.

Noble and Curtis (171) report evidence that the breeding colors of fish serve as cues to attract the same species. Fish which develop red breeding colors are attracted by red in early life while those which develop black colors are more attracted by black. The association with parents appears to strengthen the native tendency to approach disks of a particular color since isolated fish respond to them relatively slowly in comparison with fish raised with the parents. The three-spined stickleback has been studied extensively by Tinbergen and his co-



workers (175, 232) and some of his results confirm the findings of Noble and Curtis. Tinbergen (232) prefers to use the releaser concept as a working hypothesis, which he describes in part as follows:

Innate behavior is never a reaction to an environmental situation as a whole, but only to a few parts of it. Other parts—although they may be received by the animal's sense organs—are entirely ignored . . . (p. 6).

. . . only innate behavior is dependent on sign stimuli . . . every unconditioned reaction is dependent on its own special sign stimulus and different reactions respond to different sign stimuli. This shows that the release of every unconditioned reaction is dependent on a special central nervous mechanism, which is called the "innate releasing mechanism" (p. 7).

Stickleback are said to display releasers of form, color, and movement (175, 231, 232). Using various models of different forms, it was found that the red belly acts as a visual releaser. The type of movement exhibited also serves as a releaser. A model placed in the aquarium with head down elicited fighting, while the same model with head up elicited following behavior from other fish. The swollen abdomen of the pregnant female serves as a strong releaser for mating behavior. Thus, aggregations in this fish are frequently dependent upon the response of one fish to the cues provided by another.

Fisher (100) investigated the effect of temperature on trout and salmon, using a channel aquarium in which the temperature varied from 0°C. at one end to 30°C. at the other end. The fish, which had been acclimatized to 4°C. congregated at two points, the trout at 10°C. and the salmon at 14°C.

*Amphibia.* The reaction of tadpoles to light has been studied by Folger (102). Aggregations formed at the lighted end of the jar. The number of animals aggregating differed from group to group, and from day to day within the same group. Several chemicals affect this reaction to light.

A field study of the toad by Freisling (106) indicates that the geographic distribution of this animal is largely dependent upon a thigmotactic response. The skin of the thigh and of the abdomen is most sensitive to desiccation, and the toad adjusts, within temperature limits, to climatic differences in terms of the moisture content of the substratum against which the sensitive skin is pressed.

It is characteristic of the studies cited that phylogenesis rather than ontogenesis has dictated the hypotheses which have been tested. Schneirla (194) has questioned the wisdom of failing to investigate the ontogenetic development of behavior patterns in lower animals because of the belief that the behavior is natively determined. For example, Schneirla takes issue with the Lorenzian concept (149, 150, 151, 152) of a "surrogate neural center," viz., the releaser for a specific behavior pattern, and presents an analysis of the so-called released behavior in terms of simple conditioning. Apparent universality of a behavior pat-

tern is said to be in part a function of a relatively common environment containing the elements requisite to conditioning.

This theoretical approach to the study of aggregations would perhaps serve the end of pointing up the role of early learning in highly stereotyped behavior.

#### SOCIAL FACILITATION

In Crawford's review (77) of the literature, the term social facilitation was used to refer to "... increments in the frequency and intensity of responses already learned by an individual, shown in the presence of other individuals usually engaged in the same behavior" (p. 411). We shall use the term in the same manner in the review which follows.

*Fish.* There have been a number of recent reports substantiating earlier observations of the effect of one fish on another (15, 45, 223, 224). There is a lower rate of locomotor activity for a group of goldfish than for an individual (45, 223, 224), but this effect appears to be dependent upon a visual response to like-formed individuals. Allee (15) labels this group effect "automatic cooperation" or "unconscious mutualism."

*Birds.* Mowrer (165) reports that birds being trained to talk are inhibited by the presence of another bird, although talking is facilitated by the presence of the trainer. It is likely that bird calls are facilitated by the presence of other birds in a nonlaboratory situation.

*Infra-primate mammals.* A number of studies on social facilitation have been done on the rat in the past decade. Rasmussen (177) found that a social situation influences drinking for the rat, in that rats drinking in groups of three drank more frequently than rats drinking in isolation. Bruce (49) obtained similar results using water as the reward for mastering an open field problem. When a second rat was present in the field, the time for the first drink was shortened whether both animals or only one had access to the water. In a later study (50), involving variation of the drive strength, no facilitation was found.

Using food as the reward, Winslow (244) found that speed of running on a straight runway, and on two identically designed elevated mazes, was affected by the presence of a second rat. The competitive situation facilitated the behavior of some subjects and inhibited the behavior of others (in some instances to the point of a refusal to perform). The rats that were continuous early winners were facilitated by the partner, while those that were continuous early losers reduced their efforts in later trials. There were no observed differences between cage and non-cage mates. Winslow (245) has obtained similar results with cats trained to run singly, then in pairs in a 25-foot runway. Some of Mowrer's (162, 164) work on competition and food sharing in the rat bears on this problem. Mowrer finds that,

When animals which have learned individually to run a long alley for food are run in pairs, the initial effect of the resulting competitiveness is to increase

the speed of both animals slightly; but soon the slower of the two becomes demoralized (extinguished) and either stops running entirely or runs very erratically (164, p. 3).

Competition was changed to sharing by manipulating the animal-pellet weight ration. If the rat had an opportunity to escape with the food pellet into a compartment which excluded his cage mates, competition continued and no food sharing appeared. Lepley (148) failed to confirm the results of his earlier study in which rats that were paired showed a small amount of facilitation in running speed.

In all these studies of mammals the subjects have been trained to a criterion singly and then run singly and/or in pairs to determine the amount of facilitation attributable to the pairing. The amount of facilitation obtained generally has been negligible. Studies in which social facilitation is related to the learning process might reveal that this factor is of greater importance in mammalian social behavior than has been revealed by previous studies.

Using pairs of rats, one reared in isolation and the other in a group, equated for swimming ability, Bayroff (23) failed to find a significant difference between the isolate and group-reared subjects in speed of swimming underwater against a current. The factors of age, early experience, weight, and litter membership appeared to have no effect in this experimental situation.

A somewhat different type of problem situation has been used by Miller and Postman (157), Ross, Smith, and Denenberg (186), and Denenberg (88) to determine the influence of a group on the (learned?) performance of an individual. The question investigated in each of these studies was, "Will rats hoard more in groups than they will in isolation?" Using a central bin of food pellets, Miller and Postman found no significant increase in the amount hoarded to the rat's "home" cage. In the study of Ross *et al.* (186), rats hoarded under both solitary and group conditions over a period of eight weeks. The subjects were isolated from weaning until they were approximately four months of age. They were then run in isolation and in groups in a random manner so that each rat was paired with every other rat for part of the eight-week period. There was no facilitation of hoarding as a result of the group situation. Instead, a dominance relationship occurred in which the submissive member of the pair hoarded less under the group condition. Using a larger number of subjects, all reared under group conditions, Denenberg (88) found that hoarding was significantly greater under isolation conditions. This result is attributed to the social facilitation of eating under the group condition, thus reducing the amount of time devoted to pellet carrying.

Rats placed in novel situations, singly and in pairs, were not emotionally influenced by the group situation, as determined by counting and weighing the feces, according to Anderson (18).

Roubaud and Provost (188) found that sheep and goats when isolated from a group exhibited an increase in temperature and a reappearance of trypanosomes in the blood. This effect is said to be dependent on the agitation which results from the sudden change of environment. The group situation apparently has an indirect inhibiting effect on the infection.

Ross and Ross have reported observations on the social facilitation of feeding in dogs. In the first study (183), the amounts eaten in isolation and with the litter are compared. A litter of four Chow-Basenji puppies ate 14 per cent more in the group situation, while a litter of Terrier-Dachshund puppies ate 51 per cent more in the group situation. Individual performance varied from a 3 per cent increase to an 86 per cent increase in the food consumed under the group feeding situation. No reliable measures of dominance relationships were found.

In a second study (184), the effect of feeding hungry puppies on the eating behavior of satiated puppies was investigated. The results appeared to indicate that the presence of hungry puppies facilitated further eating in satiated puppies. The increase in the amount eaten by each individual varied from 30 to 200 per cent. Fellenius (99) has observed a facilitative effect on food preferences in dogs as a result of the presence of a gull in the feeding situation. Brogden (47) found no evidence of more rapid conditioning in dogs when a second dog was introduced into the laboratory.

#### IMITATION

Crawford (77) uses the term imitation to refer "to observational learning in problem situations" (p. 411). The distinction between social facilitation and imitation is somewhat difficult to maintain in certain cases. In a general review of the literature on imitation in animals, Roberts (181) concludes that imitation has been proved only in birds, where it is dependent on auditory cues.

*Fish.* In 1940, Schlaifer and Breder (225) reported finding a tendency of a social nature for tarpon to rise in imitation of manipulated models. In a later study, Breder (43) found that this imitation was less frequent in adult animals. It is doubtful that this behavior pattern in young tarpons is imitation in the sense of the definition given above.

*Birds.* Akhmeteli (1) has found that pigeons can learn food-getting behavior, without feeding, from a trained partner. The pecking movements in the trained bird appear to act as the stimulus for pecking in the untrained bird. The rate of acquisition of the behavior was slow and unstable. It is possible that this represents social facilitation without true observational learning as an important determinant.

*Infra-primate mammals.* Bayroff (24) and Bayroff and Lard (25) have investigated learning in the white rat in a water maze. The rats were required to select the correct path to a goal, the only cue to the



correct path being a rat that had received previous training in the problem. With food as the reward, all rats eventually learned to follow a leader. In another well-known experiment, Miller and Dollard (159) have demonstrated that although there was no initial tendency for the rats to imitate, they did learn that the direction which a leader takes signifies a reward. When a leader with a different color was substituted, there was still a marked tendency for this leader to be followed, but it was not as strong as the initial tendency.

Brogden (47) investigated the effect on one dog of observing another dog undergoing conditioning of forelimb-flexion, and found no evidence that the observer could be conditioned any more rapidly because of his experience.

In a series of five experiments using thirteen cats as subjects, Herbert and Harsh (135) found that on problems within the subject's range of ability, the cats benefited more from observing the learning process than they did from observing trained leaders.

*Primates.* Imitative behavior in cebus and rhesus monkeys has been investigated using the Warden duplicate-cage technique (240). A total of 144 trials was given, approximately three-fourths of these resulting in imitation on the first trial. The remainder of the trials were cases of varying degrees of imitation. There is a reasonable doubt that the stimulus pattern used in this technique was well controlled. Crawford (77) believes that the scoring of the responses made by the subjects in the duplicate-cage technique was biased in favor of imitation.

Voronin (238) has reviewed the literature on the formation of conditioned responses by imitation and attempts to show that "imitation in higher nervous activity plays an essential role in the establishment of conditioned connections" (p. 131). Some data from experiments on monkeys are used to support this point.

A series of tests used by Elder (92) to study imitation in chimpanzees appeared to reveal that a dominant animal is a better demonstrator than one subordinate to the observing chimpanzee. White (243), in an essay on the use of tools by primates, proposes that primates have failed to develop a culture, not because they cannot imitate, but because they are unable to use symbols to provide continuity of experience from generation to generation.

#### DOMINANCE, AGGRESSION, AND TERRITORIALISM

These topics have apparently continued to interest investigators more than any other subject covered in this review. There are several excellent reviews of recent work on vertebrates, the most comprehensive of which is the one by Collias (69). For other general discussions and reviews, see Collias (71, 72) and Allee (10). As it is not feasible to review every reference in detail, we shall restrict our discussion to what seem to us to be some of the more important papers. It is encouraging



to note that a much wider range of species has been investigated in the last decade than in previous years. There is some difficulty involved in avoiding references to work which has been done involving the relationship between sex and dominance, since sex appears to play an important role. The best single source of material on this aspect of the problem is Beach (27).

*Fish.* Braddock (41, 42) has reported some of the dominance relationships existing for *Platypoecilus maculatus*. Most important of these factors is the effect of prior residence (42). It was found that the intruder of a pair tended to be temporarily subordinate to the resident of the aquarium even when the intruder was larger than the resident. This relationship held for both sexes. Greenberg (115) found that 27 groups of four sunfish each, developed relatively stable dominance hierarchies. In 17 other groups several approximately equal fish set up and defended territories, thus keeping the remainder of the group subordinate. Such factors as sex, size, and conditioned learning appear to be related to dominance for this species.

Tinbergen (231, 232) and Pelkewijk and Tinbergen (175), working on the stickleback, have found that such factors as the red belly of the male, form of the other fish, and appropriate movements serve as the cues which release aggressive responses. These researches have been based on the Lorenzian concept of releasers and hence have minimized the role of early conditioning.

*Reptiles.* Greenberg (114) has made a laboratory study of *Crotaphytus collaris*, the collared lizard, and reports that only the males are aggressive. The males showed marked discrimination of sex, probably on the basis of the throat color of the male. A ten-year study of the chameleon by Greenberg and Noble (116, 117) contains some interesting material bearing upon the present topic. The males defend territories during the breeding season although there may be frequent disputes over boundaries leading to temporary dominance hierarchies. Sex discrimination by both male and female is excellent, probably on the basis of distinctive coloration, structure, and movement.

*Birds.* For general references on dominance and other social factors in birds, see Armstrong (21, 22), Lack (145, 146), and Noble (170). Extensive observations have been made on common domestic fowl by Allee and his co-workers. For a detailed review of this work, see Allee (10). Guhl (118, 119) studied the relationship between peck-order position and social discrimination in small flocks of White Leghorns and found that individual differences and previous pair contact experiences were important factors in social discrimination. Guhl and Allee (121) rotated hens through several flocks and found that hens in unstable flocks ate less, lost weight, and laid fewer eggs. Stable dominance relationships within a flock would seem to have important survival values (10, 123). In a later report, Guhl (120) confirmed the fact that new birds

added to a flock upset the flock temporarily. Douglass (91) found that recently added hens which were high in dominance were integrated into the flock within a few days. Hens of low dominance rank were not admitted for several weeks. Hens may be members of several flocks simultaneously and hold a different rank in each flock (90).

Guhl and Warren (122) report that dominant cocks mate more frequently and sire more chicks than their subordinates. Allee *et al.* (12, 13, 14) have investigated the effects of estradiol, testosterone, and thyroxin with varying results on dominance. Collias (68) has studied the factors determining success in initial encounters in hens, holding social facilitation, sex, and familiarity with the fighting pen constant, and reports that success is related to male hormone output, thyroxin, weight, and rank in the home flock. These four factors gave a multiple correlation coefficient of plus .75 with initial success in fighting.

Hale (124) observed debeaked hens and found no indication of lessened social tensions. The debeaked hens were successful in maintaining social position and in winning fights. There was a higher frequency of pecks in the debeaked hens than in normal hens.

Ringdoves have a social organization like that of pigeons, according to Bennett (33). The social order is purported to be dependent upon recognition factors although no permanent effects occurred from dyeing the plumage and altering the contours of the doves. Testosterone-injected males and females were found to advance in social status (32).

Dominance orders and territorial factors have been studied in several birds under field or near field conditions. Jenkins (143) has observed interspecies dominance for several groups of geese and wood ducks. Juncos and tree sparrows have been studied by Sabine (189) as have quail by Howard and Emlen (130). Other groups studied include chickadees (127), penguins (46), grouse (216), pheasants (70), Hawaiian birds (17), and White Pekin ducks (11).

*Infra-primate mammals.* Burt (53) has reviewed the literature on territoriality and discussed some of the concepts of this and the home range phenomena.

An extensive literature on dominance relationships in highly inbred strains of mice has appeared over a ten-year period, much of this work being done at the R. B. Jackson Memorial Laboratory, Bar Harbor, Maine. One of the most useful studies was done by Warne (242) who made a time analysis of 17 groups, each containing four male mice. The observations were made over a period of a year and a half. The results showed that only .24 per cent of the observation time was spent in actual fighting, indicating that this activity is relatively minor in the daily routine of these mice. It is interesting to note the dominant mice spent more time in grooming others than did their subordinates.

Scott (195, 196) has investigated genetic differences in social behavior for several strains of mice, and found indications of hereditary differences in fighting behavior. Territoriality in mice is said to be of

value chiefly in controlling population density (202). Territories in this study were fluid and variable, not at all well defined. Scott has proposed that social behavior determines social organization (200, 203, 205), i.e., knowing the social behavior of given subjects, one can predict the type of social organization which will be formed.

Ginsburg and Allee (112) have investigated the effects of conditioning on dominance-subordination in mice. They have succeeded in demonstrating that a mouse can be conditioned to be either more or less aggressive, although it is easier to condition for less aggressiveness. Inexperienced mice can be conditioned to a low status by interfering with their adjustment to trained fighters (207). Scott (208) has found also that mice presented first with a trained fighter and then with helpless mice attack more frequently than mice presented only with the helpless mice. Uhrich (236) has found that previous experience was more important for albino mice than social status and weight in the outcome of a fight. Fredericson (105) succeeded in inhibiting fighting by stabilizing the primary reinforcing stimulus, food, by securing it in a permanent position. The effects of food deprivation (104), thiamin (31), amphetamine (65), and male hormone (30) on the fighting behavior of mice have also been investigated.

For an extensive review of the work on dominance in rats, the reader is referred to Munn (167), who considers in detail the work of Miller and Dollard (159), Miller (158), and Seward (218, 219, 220, 221). One of the interesting studies of the Norway rat in a natural habitat has been done by Calhoun (54, 55). A square area with 100-foot sides, surrounded by a rat-proof fence, was used in this study. Rats were tagged and marked for observation from a tower. At the time of publication, the study had been under way for eighteen months. Of importance here are these observations:

... rats born near the source of food grow more rapidly, and, conversely, those born distant from the food source grow more slowly; the latter, as a result of the fact that greater weight is a deciding factor in combat, are forced to remain in submarginal areas, (with reference to the central food source) and their young in turn tend to assume a low social status. Group continuity in space and time is of value in reducing strife (56, p. 1122).

Collias cites an unpublished study by Heath (132), in which early weaned rats exhibited significantly more aggressiveness than rats that remained with the mother. There was no significant relationship between weight and aggressiveness. Rats raised in groups were found to "freeze" more frequently in problem situations than rats reared in isolation (180). Nearly all of the "freezers" were submissive in fighting in the cage situation. Hall and Klein (125, 126) have found aggressiveness in rats to be a relatively stable characteristic even when the rat is paired with another in the latter's home cage. Billingslea (36, 37, 38) reported on a series of experiments on the relationship between emo-

tionality and some other behavior patterns, including aggressiveness. Related to this is Daniel's critique (84) of the O'Kelley-Steckle reaction. Daniel found that rats were stimulated to fight while being shocked, but little fighting occurred following shock termination. Defense postures were common in the postshock period. A similar result occurs following injections of amphetamine in mice, according to a study by Chance (65). In another study, Bruce (51) found that a rat that was dominant under food motivation was also dominant when motivated by thirst. Dominance occurred only when a competitive element was present. Fredericson (105) has reported similar results for mice. A film by Mowrer *et al.* (166) shows the development of a dominance hierarchy from the stage of no fighting, through competition, to savage fighting as hunger increases.

James has continued his study of dominance in dogs and has found that shifting dogs of known social status in one group to another group led to changes in rank only for those dogs in the middle ranks in the original group (140). In a later study, James (141) attempted to change the status of an underweight, submissive member of a litter by special handling and feeding. Even when the submissive dog equalled the dominant animals in weight, his social status remained the same. Several months in a submissive role apparently cannot be overcome by simply reducing the weight differential. This particular litter of four puppies had developed a food dominance differential by 85 days of age (142). This relationship was maintained until 115 days of age, when the one female became dominant over the three males. Berg (34) found that chronic B<sub>1</sub> deprivation leads to a lowering of social status. This status was regained after a series of B<sub>1</sub> injections.

Scott (211) has investigated genetic differences in the social behavior of two breeds of dogs, Basenjis and Cocker Spaniels. A standardized handling procedure was followed and all reactions were recorded at 5, 7, 9, 11, 13, and 15 weeks of age. Scott concluded that any differences between the two groups were genetically determined. In his studies of the development of social behavior in dogs, Scott has observed the development of dominance in litters of puppies (215). Vogel (237) has reported a study of social relationships, including dominance, among Springer Spaniels. The Menzels (156) have observed the behavior of dogs and cats of varying degrees of tameness and found that aggression ranged from mutual tolerance to constant fighting.

In studying experimental neuroses in dogs, Anderson and Parmenter (19) found that dogs which were in a state of anxiety had a low social status in the group.

The lair dwelling behavior of dogs has been observed by Ross (182). Daily defecation records were kept and these indicated that defecation tended to take place in approximately the same cage area every day. In the case of dogs that had access to an outdoor exercise cage, defeca-



tion never occurred in the living cage during the period of observation. This defecation behavior is undoubtedly related to territorial factors. Hyman (139) reports that hyenas in captivity have been observed to smear an anal gland secretion around the cage and pen. This is probably related to territory demarcation.

Winslow (245) trained cats singly to run in a runway, in a problem box, and in a string-pulling problem. When cats were run in a group of three, displaced aggression was observed. The more dominant loser commonly attacked his subordinate. An experimental study of dominance hierarchies for four groups of four cats each has been reported by Masserman and Siever (155). Once a hierarchy was formed, the alpha animal did not become aggressive until displaced by another animal or until a neurosis occurred as a result of motivational conflict. When drugs were administered, or the neurosis released, the animal regained his alpha status.

Dominance reactions in a flock of goats are described by Scott (206). The degree of aggression for each member of the flock was classified on a five-point scale ranging from 1, absence of aggressive behavior by either one of the pair, to 5, aggression by one goat with no resistance from the other. Stewart and Scott (228) report a study of leadership and dominance in which the criterion of dominance was the number of fighting acts, whether aggressive or submissive. Using the paired comparison method, with a restricted feeding technique, Scott (209) has attempted to test the frustration-aggression hypothesis on goats. The flock was composed of fourteen goats of several ages, both males and females. Frustration is said to occur when a hungry goat is prevented from eating by the presence, in a restricted feeding situation, of a more dominant subject, i.e., one who eats the limited quantity of food. The effect of delaying the feeding and recent feeding in relation to the degree of dominance was investigated. It is proposed that aggression can be controlled by controlling the amount of food given. Increasing the feeding decreases aggressiveness. Frustration (in feeding) leads to aggression only in those animals that are "in the habit of being aggressive." The relationship between dominance and control of movement was studied by Ross and Scott (185). It was concluded that dominance in goats is not an absolute form of social control, since it resulted in definite control of the movements of other animals in only 0.7 of the cases. The importance in dominance of such factors as age, weight, sex, and hornedness was confirmed.

Scott (197, 198, 199, 201, 204) has reported several investigations of the social organization of domestic sheep. The social behavior of sheep is considered in terms of mutual imitation, epimeletic behavior, signaling for care and attention, sexual behavior, and fighting. Very little fighting over food took place in the flock of sheep, although fighting among rams when a ewe was in heat was common (204). He says, "It



appears that rewards, training and stimulation of innate behavior patterns were far more important types of control in this flock of sheep than was dominance. . . ." (p. 18). Katz (144) has demonstrated a dominance order in a herd of Barbary sheep. The male was dominant over both females and young. The weight and horns of the male appear to be important determinants.

That the "hook-order" of cows can be changed by removal of the horns has been reported by Woodbury (247). If all the horns of the herd are removed, a more peaceful social organization based on a "butt-order" develops. A dominance hierarchy among lions in an outdoor enclosure was observed by Cooper (75). Burt (53) and Gordon (113) have reported observations on dominance and territorialism in squirrels. The formations of herds and the importance of threatening postures and movements in zebras, horses, and mules are described by Antonius (20). Schenkel (190) reported on the importance of expressive behavior in wolves in captivity. The behavior of a muskrat population subjected to periodic drought over a fifteen-year period has been described by Errington (96). As the environment became more intolerable, aggressiveness increased markedly and territories broke down. Brown (48) reports a relationship between sexual dominance and the magnitude and stability of a conditioned response. When reversals of sexual dominance occurred, changes in the conditioned response also occurred.

*Primates.* Excellent studies of primate groups have been done by Carpenter (57, 58, 60). This investigator is also responsible for four methodological and theoretical papers on primate societies (59, 61, 62, 63). Several papers are available concerning the relation of sexual status to dominance (39, 40, 66, 67, 174, 248, 249).

Maslow's report (154) on infrahuman primates includes a comparative discussion of anthropoids, and Old World and New World monkeys. New World monkeys are relatively low in dominance while Old World monkeys are extremely aggressive, this being exhibited in bullying and assault. Chimpanzee dominance is reported to be playful and friendly. The behavior of paired green monkeys in problem solving is dependent almost entirely on the dominance relationship between the subjects (93).

A study of several social factors has been made by Warden and Galt (241) for nine monkeys: four cebus, three rhesus, and two sooty mangabeys. Two baited boxes with cords attached had to be pulled in by the paired subjects. A four-point dominance scale was used for rating purposes. These writers report: "The dominance factor showed itself both in the attitude of an animal to its mate and its reaction toward the food. The former might include threatening, striking, and fighting activity, and the latter an attempt to pull in both boxes and secure all the food" (p. 219). Interspecies pairings on the problem resulted in the rhesus monkeys' being dominant over the cebus in practically 100 per cent of

the trials, and dominant over the mangabeys in the majority of trials. Cebus and mangabeys, when paired, resulted in higher status for the mangabeys in five out of six cases. Interspecies dominance was more stable than that within the species.

A study of the social behavior of eight female chimpanzees in a non-competitive situation indicates that of the many types of behavior exhibited, only frequency of grooming was related to dominance in a feeding situation (78, 81). Crawford (80) found that when chimpanzees that have been separated for varying intervals are introduced into the same cage, the animal that enters the other animal's cage is groomed first, and the one that attacks or threatens attack is likely to be dominant in a food-division test.

Montagu (161) reports that he has found an almost perfect relationship between body-size, waking or sleeping habits, and the origins of social life in primates. The appearance of dominance patterns appears to be related to a shift from nocturnal habits.

An extensive study of preference for a companion and dominance has been made by Nowlis (172) for five pre-adolescent chimpanzees. The test situation consisted of permitting a test animal to approach one of two stimulus animals and to have contact with the animal approached. Two series of tests were conducted; the first when the animals were kept in isolation at all times, except during the testing, and the second when the animals were isolated only during the day. Dominance was determined by a competitive food situation and by priority of escape from a strange situation. No relationship was found between dominance and companion preference. Nowlis states that: "... The behavior of the subjects suggests that aggression is the most important factor in their competitive behavior and in the development of relative dominance status. ... The animal which consistently competes successfully by means of aggression directed towards its partners acquires habits of approach and aggression in competitive situations and furnishes conditions for the development in its partners of habits of withdrawal from punishment and from competition" (p. 52).

The relation of sexual status to dominance is discussed in a paper by Yerkes (249) based on a study of twenty-one pairs of adult chimpanzee mates. He says: "The basic principle of relationship revealed by the experiment involves sexual status. When a female is sexually receptive it is her *right* (italics added) to claim control of the situation without interference by her mate. ... Sexual status, then, appears to be the most important single determiner ... in the experiment" (p. 185).

This pattern of behavior also has been observed by Carpenter for primates other than chimpanzees (59). Finally, Nowlis (173) has studied the relationship between degree of hunger and competition in chimpanzees. The effects of satiating the dominant partner or the submissive partner is considered.

FAMILY LIFE<sup>1</sup>

*Infra-primate mammals.* Reference has been made previously to the research program of the Jackson Laboratory where part of the effort is being directed toward a long-term study of the development of the dog (108, 187, 210, 211, 226). By standardizing the handling technique, a relatively constant environment is provided, against which genetic differences in behavior can be compared. Royce's (187) factorial study of several physiological, psychological, and social measures, and Scott and Marston's (214) discussion of critical periods of development are both related indirectly to the family life of the dog. Extensive observation of several litters of dogs has led Scott and Marston to propose that those periods in the development of an organism in which new social relationships are formed are "critical" in the sense that the habits formed in the critical periods are likely to be carried over into later life. The experimental program testing this hypothesis is just now starting. The only other study in this group is by Seward (217), who has pointed out the importance of hunger in the filial behavior of guinea pigs.

*Primates.* Carpenter (59) has summarized his observations on grouping patterns and family relationships for several nonhuman primate societies. This writer's observations on play and its relationship to aggressiveness also are of importance. Beach (26) has reviewed some of the current concepts of play. (See Shadle, 222; also.) Spence (227) reports that five out of seven mother chimpanzees were able to recognize their own children when the child was presented along with an unrelated offspring following varying periods of isolation.

## EFFECT OF ISOLATION ON SOCIAL BEHAVIOR

*Infra-primate mammals.* The studies of Denenberg (88) and Ross *et al.* (186) on group hoarding indicate that rats reared in isolation and hoarding in groups were more overtly aggressive than rats reared in groups and hoarding in groups. Heath's (132) study of rats isolated early from the mother is also pertinent to this point. Scott (204) notes that two lambs isolated from the flock and bottle-fed were not accepted by the mothers upon return to the flock.

*Primates.* Reference has already been made to Crawford's (80) study of pairs of chimpanzees isolated from each other for from five weeks to three years, and to Nowlis's (172) study of companionship preference which involved temporary isolation from the group.

## COMMUNICATION

A review of the types of communication that may exist among animals and between man and animals has been presented by Révész

<sup>1</sup> A stimulating discussion of the development of family life in infrahuman animals has been given by Schneirla (194).

(179). Mowrer's (165) chapter on the psychology of "talking birds" is a particularly provocative presentation of the technique of teaching birds to talk and the relationship of this behavior pattern to the development of language in the human. It is proposed that the birds reproduce those sounds which "sound good to them" (p. 699). The acquisition of words is considered in terms of primary and secondary reinforcement. Hayes (131) has reported in abstract on her work in training a chimpanzee to speak. Allard (2) has made field observations on starlings and mockingbirds and has found an unusual range of sounds which normally occur in the summer environment being reproduced "out of season," as it were.

Carpenter (57) recorded gibbon calls in the field and then played some of the recordings back. Vocalization is said to be an important part of the behavior of this group. Although no detailed analysis of gibbon calls has been reported, Carpenter does note that, "In general it seems that there are five types or series of sound patterns. Each series is distinct, is produced in a specific situation and produces in associated animals characteristic responses. Several times it was possible to stimulate the wild animals to call by playing back to them the recordings of their own calls which had just been made" (p. 325).

#### RECOGNITION OF KIND

Several references have been made in this review to factors which appear to be determinants of the ability of some fish, lizards, hens, mammals, and infrahuman primates to recognize other members of a group. This recognition is most frequently on the basis of distinctive coloration, form, movement, or vocalization. In an infrahuman primate group there appears, according to Carpenter, to be a recognition by every member of every other member of the group, although this has not been experimentally demonstrated.

Howells (136) and Howells and Vine (137) in a study of social learning have contributed some data on this problem. Incubator-hatched bantams and leghorns were reared in mixed groups, thus giving each member of the group opportunity for equal experience with bantams and leghorns. When the subjects were three weeks of age, they were trained in a Y discrimination box with certain members of its flock as cues. The chicks learned to go to their own kind more readily than to the non-like bird. This effect is interpreted as "a product of a differential influence on the learning process exerted by the innate factors" (137, p. 547). Bantams learned to go to the bantams more readily than leghorns learned to go to their own kind, perhaps because of greater activity in the bantams.



## COOPERATION AND FOOD SHARING

*Infra-primate mammals.* Daniel (82, 83, 85, 86) has conducted several studies on cooperation in rats. The problem was arranged so that a pair of rats could escape an electric shock by cooperating in a feeding situation (83). The individual animals were trained in the problem so that it was essentially a case of rats learning to cooperate. As the rats worked in the problem, they tended to pay more attention to each other so that exchange of position from food to platform was accomplished frequently enough to provide satisfaction of both the motives of hunger and escape from shock. An extension of this study (85) indicated that cooperation could be maintained with low shock or threat of shock but would disappear with no shock at all.

In a well-known film, Mowrer (163) has shown that three rats faced with a problem of manipulating a bar at one point in order to obtain food at a bin at another point established a "parasitic" relationship rather than a cooperative one. Tsai (235) reports that rats learned to cooperate in a problem requiring the simultaneous pressing of separate keys in order to raise a door to secure food. Rats also learned to press a key to release a partner from a detention box before proceeding with the remainder of the problem. In a problem requiring rats to pull a cord together in order to obtain food, Winslow (244) found no clear evidence of cooperation.

Mowrer (162, 164) reports that in one experiment feeding behavior in rats passed through several stages, including food-sharing. If deprivation is increased, competition becomes more intense and sharing does not develop.

*Primates.* Monkeys failed to exhibit spontaneous cooperation in a problem requiring the pulling of a weighted box too heavy for either member of the pair to move singly (241). The animals failed to cooperate following tuition, contrary to results obtained for chimpanzees. Wolffe and Wolffe (246) have studied cooperative behavior genetically in a comparison of monkeys and children. Crawford (79) studied the ability of six young chimpanzees to cooperate in solving a serial response problem. Varying degrees of cooperation were shown.

Carpenter and Locke (56) report the development of trading responses in a cebus monkey which learned to exchange a specific object for food. This behavior appears to be related to a type of sharing. Nowlis (172) found that food transfer occurred from a subordinate to a dominant animal. There was no relationship between the sharing of food and preference for a companion. Yerkes (250) reports that food was shared by partners only when the female was in estrus and took the food as her right.

## GROOMING

Warden and Galt (241) report that unilateral brain injury decreased grooming in monkeys and bilateral injury completely eliminated groom-



ing. Grooming often took precedence over food seeking and did not seem to be related to sex or dominance status. Crawford (80) found that one index of dominance status was grooming, the grooming being done by the subordinate animal. In young chimpanzees "... 87 per cent of the social behavior occurred in the form of two distinct patterns, grooming and play-fighting" (172, p. 33).

Reference should be made to several other studies related more or less directly to the social behavior of vertebrates. Attention might be called to Hebb's (133, pp. 245-250) description of mental illness in chimpanzees since this appears to be social in nature. Some of the work on neurotic behavior (110, 153, 156) has important implications in social development. Hebb's (134) proposed methods of analysis of temperament are of interest. The effect of a conflict situation on pairs of chimpanzees is described in an abstract by Fletcher (101).

Finally, we can include here Bechterev's (29) report on the "influence" of man on the behavior of animals, and Curti's (74) report on the responses of albino rats to the presence of a cat. Visual cues are apparently not necessary in order for the rat to recognize a cat.

#### SUMMARY AND INTERPRETATION

Numerous papers in recent years have presented various theoretical interpretations of social behavior. The most popular "explanatory" principles have been those of cooperation and competition (3, 5, 6, 7, 8, 9, 10, 94, 95, 109, 160), both of which have sometimes been used as intervening variables and at other times as dependent variables. There have been theories based on releasers (Tinbergen, 229, 230, 231, 232, 233) (Lorenz, 149, 150, 151, 152), trophallaxis (191, 192), and others in which learning (76, 137, 138, 158, 159) is the primary principle proposed as a basis for a theoretical comparative social psychology. Each of these theories has some merit, but all suffer from too heavy an emphasis on a narrow range of methods and problems for which their theory has some "face" validity.

Beach (28) has pointed out the need for a comparative study of social behavior and for the advantage of using a wide variety of species as test subjects. It is difficult to see how there can be a comparative social psychology unless and until a wider range of animal subjects is studied. But the use of more species will be of relatively little value unless the investigations undertaken are guided by some systematic theoretical framework such as have been proposed for learning by Hull and for personality by Sears. A framework of this kind might entail the formulation of a number of postulates and the systematic investigation of the intervening variables which are deduced. Unifying principles

are urgently required if such studies are to have a major applicability to the problems of human social behavior.

In addition to deficiencies in theoretical orientation and in the range of species studied, there also exist weaknesses in the techniques used in the study of social behavior. There is a need for new approaches to existing problems, and for new problems to be conceived and studied. The use of models by those individuals investigating the concept of social releasers represents an attempt to meet such needs. It is very possible that much of social behavior is situational; hence the range of experimental situations used might well be increased far beyond runways, mazes, and problem boxes. One of the most promising attempts at the solution of such problems is to be found in the symposium on the "Methodology and Techniques for the Study of Animal Societies" (212). This report documents an attempt to approach the study of social behavior in a systematic manner by delineating problem areas and by suggesting appropriate techniques to be used.

Finally, the range of problems studied has been relatively limited, as can be seen easily by examining the various areas for which work is cited in the review. Little has been done on such important phenomena as play, communication, imitation, and learning in social situations. These phenomena are of great interest and perhaps the next decade of research will provide more significant advances in our knowledge.

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## BOOK REVIEWS

HULL, CLARK L. *Essentials of behavior*. New Haven: Yale Univer. Press, 1951. Pp. viii+145. \$2.75.

The eight years between the publication of *Principles of Behavior* and *Essentials of Behavior* have been busy ones in the field of learning and conditioning. New experimental data have been accumulating at an impressive rate, at the hands of both Hull and his followers, who were testing the system, and other researchers who were working without reference to the way in which their studies might bear on Hull's system. In the face of this new knowledge, many of us have wondered what changes had occurred in Hull's thinking over these years. The present book is the answer, or at least part of the answer.

This is a little book, scrupulously stripped down to a crisp and pithy presentation of its core ideas. It offers a restatement of the earlier system of the *Principles*, with the embedding contextual material reduced to a near minimum. The author says: "During the last eight years the fundamental hypotheses of my system have matured considerably. This development will be reflected in the system itself, which I hope to publish within a year or two" (p.v). Hull was responsive enough to the new data to feel that public emendation of his earlier system was called for early, though he apparently had a more thorough recasting already in mind.

Besides containing the latest statement of Hull's theory of behavior, it was the author's hope that the present book would be both "intelligible" and "a useful introduction to the current aspects of behavior theory." Newcomers to Hull, however, will probably find that the conciseness of the book is gained at the cost of intelligibility. All theories require a certain amount of plain language context to be comprehensible. This little book does not have enough, and a reader must be prepared to draw upon the context of Hull's earlier writings, particularly the *Principles*. This will be no hardship for the cognoscenti, who simply want Hull-to-date, and who may welcome the brevity of the book; but others, not yet familiar with Hull, will need to learn the earlier work before they can assimilate this latest one. For the same reason, and apart from any questions of the systematic adequacy of the theory itself, the book will not serve for newcomers the introductory purpose which its author had in mind.

The similarities between the *Principles* and the *Essentials* are deep-going, and compose a fundamental continuity between the two books. Here is the same philosophy of science, the same theoretical orientation to psychology, the same approach to theory construction. Hull's version of postulate method, still in force, seems to have undergone no refinement since the *Principles*, just as his faith in it has undergone no weak-

ening. Curve-fitting is still used, in place of rational equations, not only for summarizing a plot of empirical data, but for isolating many of the postulates entering into the theory. The system, in many respects, is still much as it was, in substance as well as form. It takes its departure from concepts like need and survival; the same variables are featured; the same few types of arbitrary functional relations do most of the work; some of the old postulates reappear almost intact. Hull is still preoccupied with quantification because his belief is unshaken that only in that way can "scientific" progress be made by behavior theory. Comments on these aspects of Hull's theory may be found in previous reviews of the *Principles* and in critical papers on Hull's general system and methodology.

Of more current interest are the changes that mark this new version of Hull's system. A few of the broader differences may be cited to show the scope of those changes. Hull says, "... numerous reasons for changes and modifications in the postulates as presented [in the *Principles*] have been revealed." Accordingly, he continues, "... the mathematical aspects of many of the postulates have been formulated, or reformulated, and the verbal formulation of nearly all has been modified to a certain extent. One postulate... has been dropped in part as empirically erroneous; some postulates have been divided and others have been combined; several new postulates have been added; and a number of the original postulates have been deduced from others of the present set and appear as corollaries in these pages." All this reshuffling has led to considerable trimming down in the total number of postulates, subpostulates, major corollaries, and minor corollaries to only a little more than half their number in the *Principles*. Part of this reduction, it may be noted, is achieved by omitting certain topics like stimulus patterning and *S-R* asynchronism. There has been a shift in emphasis from  $sH_R$  to  $sE_R$ , the latter being described now as "the central quantitative concept of the theoretical system," and an attempt is made several times to rationalize this shift. Much attention is cast upon the problem of reaching a quantification of "true"  $sE_R$ , with Thurstone's Case III taken as the key procedure; and the concept of probability of response is applied to the interpretation of  $sE_R$ . With regard to basic viewpoint, too, the *Essentials* offers some interesting compromises and corrections. Thus, to criticisms of his physiologizing in the *Principles*, Hull replies that a term like afferent neural interaction is "at bottom purely molar," and he points to the "methodology put forward for quantifying afferent stimulus interaction" as proof of its molar character. Or, again, he addresses himself anew to the question of whether there are one or two types of conditioning, reasserting his belief that there is only one on his customary contention that reinforcement or drive-reduction is involved in all cases. (The term "law of effect" is not mentioned once.) At the same time, he compromises by

harking back, and giving weight, to an issue which figured in the Skinner and Konorski-Miller discussion, namely, the *de novo* character of CR's obtained with one conditioning procedure and not with the other.

Comparisons such as we have been making between the *Principles* and *Essentials* are, however, a matter of historical interest mainly. What is more important is that we have in the latter a system of behavior brought up to date, presumably complete in itself, and calling for a critical inspection without reference to its earlier forms. To make this inspection fully in our space is impossible, but it will perhaps be worth while to report briefly on one part of the theory as an example of his treatment. For this, the handling of  $sE_R$ , and the variables constituting  $sE_R$ , may be chosen.

As in the *Principles*, the necessary and sufficient conditions for the acquisition of new *S-R* connections or for the strengthening of old ones is the occurrence of reinforcement. Reinforcement is said to involve drive stimulus reduction rather than need reduction. Nevertheless, it is clear from Hull's remarks (e.g., p. 15) that the development of a state of need is still considered essential, since it underlies drive and the consequent production of drive stimuli. Hull appears to be as far as ever from an operational definition of reinforcement. Postulate III, which embodies the "law of reinforcement," fails to clarify the matter. It includes a mixture of terms dealing with observable (e.g., Response) and unobservable (e.g.,  $S_D$ ,  $s$ ) events and specifies only that drive stimulus reduction must be "rapid." Obviously, this qualification leaves much room for ad hoc explanations of the failure of an apparently "reinforcing state of affairs" to strengthen behavior, since the hypothesized drive stimulation is internal, and no rules are supplied for measuring it, let alone the rapidity of its diminution.

$sH_R$ , which is generated by the reinforcement process, and drive ( $D$ ) are the key variables which determine reaction potential ( $sE_R$ ). As stated earlier,  $sE_R$  is the "central theoretical construct" of the system, but it is at the same time asserted that the procedure involved in the quantification of  $sE_R$  constitutes its operational definition. Since this quantification is specifically based upon latency measurements, this makes  $sE_R$  essentially a derived response measure rather than a theoretical construct. Thus, in view of the above assertion, equations relating  $sE_R$  to theoretical variables like  $sH_R$ ,  $J$ ,  $D$ , etc. might be converted to the form  $R=f$  (intervening variables), while the equations relating response measures like amplitude to  $sE_R$  might take the form  $R=f(R)$ .

Hull's procedure in handling  $sE_R$  is the same as that utilized in the *Principles*. Explicit functions relating variables like  $sH_R$ ,  $D$ ,  $J$ , etc. to environmental factors are written first. Then  $sE_R$  is related to the intervening variables by general equations stated in functional notation form [e.g.,  $sE_R=f(D \times sH_R)$ ], though Hull sometimes omits the " $f$ "



symbols (cf. equation 19, p. 59). Finally,  $sE_R$  is tied up, again by explicit functions, with such response measures as latency, amplitude, number of unreinforced responses in extinction, etc. It may be noted incidentally that the latter equations, written in the form  $sE_R = f(R)$ , would be more appropriate in the form  $R = f(sE_R)$ , since the amplitude, latency, etc. of response presumably depend upon  $sE_R$  as determined by the values of  $sH_R$ ,  $D$ , etc., and, in the final analysis, by the values of  $N$ ,  $h$ ,  $w$ , etc., the antecedent empirical variables. Special status is, however, given to latency measurements through their use in the quantification or operational definition of  $sE_R$ .

Of the intervening variables upon which  $sE_R$  depends,  $sH_R$ ,  $V_1$ ,  $K$  and  $J$  are functions of conditions at the time of acquisition, whereas  $D$  and  $V_2$  are considered to exert their influence during performance and extinction. That  $D$  plays its role during performance or extinction is an inference based upon Hull's derivation of the basic equation  $D = f(h)$  from extinction data supplied by Yamaguchi. However, the matter is somewhat confused by the wording of Postulate IX (p. 59) to the effect that "the reaction potential . . . is determined (1) by the drive ( $D$ ) operating *during the learning process . . .*" (Our italics.)

As noted earlier, the most important acquisition variable determining  $sE_R$  is  $sH_R$ . According to Hull,  $sH_R$  depends primarily upon  $N$ , the number of reinforcements. The so-called "law of habit formation" deals with the relation  $sH_R = f(N)$ . The latest version of this law appears to be more restricted than the one presented in the *Principles of Behavior*. It now deals with reinforcements occurring at "evenly distributed intervals" (cf. Postulate IV, p. 32). At least three points should be noted concerning Postulate IV: (a) the law omits the case of unevenly spaced reinforcements; (b) partial or intermittent reinforcement continues to be ignored; (c) neither in this postulate nor elsewhere in the volume is there any systematic mention of spacing as a parameter of acquisition.

$J$  and  $K$ , like  $sH_R$ , play a new role in the current version of the system. Instead of influencing the upper limit of  $sH_R$  as a function of  $N$ , they have become direct determiners of  $sE_R$ . As was true for  $sH_R$ , the postulate relating  $K$  to its empirical variable is much more restricted than the equivalent statement in *Principles*. Postulate VII (p. 51) deals with  $K$  merely as a function of  $w$ , the weight (gm) of food. It fails to consider non-food reinforcement, or the quality of reinforcement, both covered by the earlier postulate. The derivation of the equation relating  $K$  to  $w$  is based primarily upon data supplied by Crespi. While admitting that only three of the Crespi curves were sufficiently regular to permit empirical curve fitting, Hull proceeds to use asymptote values based upon five. He further omits the asymptote value for the zero incentive curve on the ground that it is "presumably distorted upward by frustration" (p. 49). It is then assumed, without

further explanation, that the three highest data points graphed in Fig. 12 correspond to the coefficients of the empirical equations fitted to the three suitable Crespi curves, "except that they all have considerably smaller values."

In deriving the delay of reinforcement equation (Postulate VIII, p. 56), there is again some tendency to blink the data. When equation 15, basic to Postulate VIII, is fitted to the data provided by Perin's study, the resulting curve indicates an asymptote at an above-zero value of  $sE_R$  (see Fig. 14, p. 55). Yet in the Perin study, animals failed to condition at a 30-sec. delay, denoting zero value of  $sE_R$  at that delay. This indicates either the existence of a discontinuity requiring a second function starting at some value above 10 sec. (the last plotted point), or at least the need for a more complex function, but the discrepancy is dismissed as "probably due to some artifact."

Among the intervening variables which together constitute reaction potential are two which do not appear in any form in the earlier volume. These are  $V_1$  and  $V_2$ , stimulus intensity dynamism variables for acquisition, and for performance ("response evocation") or extinction, respectively. That is, the intensity of the stimulus at the time of conditioning, and during later response evocation, is considered to affect response strength apart from all other variables.  $V_2$ , the performance variable, affects  $sE_R$  directly, being given equal status with  $J$ ,  $K$ , etc.  $V_1$ , on the other hand, is considered to affect  $sH_R$ , and a new symbol,  $s\bar{H}_R$ , is created to represent the resultant habit strength. But, according to equation 10' (p. 45),  $s\bar{H}_R = sH_R \times V_1$ , and according to equation 19 (p. 59),  $sE_R = D \times V_2 \times \dots \times s\bar{H}_R$ . Thus, by substitution, we get  $sE_R = D \times V_2 \times \dots \times sH_R \times V_1$ , and the reason for the different status of  $V_1$  and  $V_2$ , or for the creation of the symbol  $s\bar{H}_R$  becomes difficult to see.

In the attempt to demonstrate the need for stimulus intensity variables, Hull leans heavily upon an unpublished study by Hays (pp. 41-43). Apart from the admitted irregularity of the data for the group jumping to a white target, no reason is offered for the fact that the group jumping to a black target should have demonstrated virtually no conditioning in spite of 143 reinforcements spread over 13 days, and the reader is left to conjecture what the cause of the demonstrated difference might be. Whatever the cause, the study appears to provide a rather tenuous basis for the introduction of new intervening variables. Several further thoughts must occur to the reader regarding the theoretical constructions surrounding  $V_1$  and  $V_2$ .

1. From the equations fitted to the Hays data, the expression  $sE_R = f(V_2 \times s\bar{H}_R)$  is derived. But  $V_2$  applies to "response evocation," while  $V_1$  acts during "original learning." There seems to be no clear reason for treating the Hays study as being of the former case. Furthermore, the problem of deciding when "original learning" ends and "response evocation" begins would seem to be a difficult one at any time.

2.  $V_1$  is introduced in equation 10' without any attempt to demonstrate the need for such a variable at either the experimental or the theoretical level. (As noted above, Hays' study is used in connection with  $V_2$ .)

3. The assumption concerning Cattell's unit (p. 44), used to derive equation 10, seems completely arbitrary. This, together with other similar instances in the book, makes the reader pause to consider the general question of the legitimacy of making assumptions, however arbitrary, because they lead to equations which fit well data for whose fit the assumptions were created.

4. The absence of separate equations relating  $V_1$  and  $V_2$  to the empirical variable,  $S$ , presumably means that the equation in Postulate VI holds for both cases, but this is never stated explicitly and the reader is left to make his own inference.

The final intervening variable, Drive ( $D$ ), which contributes to  $sE_R$ , is sometimes considered by Hull to be the most important of all (*vide*, p. 58). As noted earlier,  $D$  presumably refers to drive at the time of performance or extinction. However, some above-zero value of drive is required also during conditioning, on Hull's assumption that reinforcement is a drive (or drive stimulus) reduction process. Some lack of clarity is evident in the discussion of drive. At times drive is described as a form of stimulation (p. 15); at other times as a need (pp. 3-4, 15); at still other times as a state (p. 15). Possibly the author intends these terms to be taken as synonymous, but there are occasions when they seem to have separate referents (e.g., p. 39, legend of Fig. 4, p. 18), and the matter is never quite settled. In fact, on p. 76, drive reduction is referred to as only "*one of the necessary conditions of reinforcement.*" (Our italics.)

In spite of such verbalizations, one role of drive in the system is unmistakably that of an intervening variable (*vide*, p. 33). As was the case with  $sH_R$  and  $K$ , the drive postulate is quite restricted in scope. With the exception of parts  $C$  and  $D$ , it deals solely with the hunger drive. Parts  $A$  and  $B$  of the postulate, as well as equation 7, are taken directly from data supplied by Yamaguchi. The latter investigator derived an equation relating  $sE_R$  to  $h$ , hours of food deprivation. In the book the same expression appears as equation 7, but relates  $D$ , not  $sE_R$ , to  $h$ . Such a substitution would be justified only if  $D$  and  $sE_R$  were the same, or if  $D$  were a close approximation of  $sE_R$ . The tendency to speak of drive as though it were some form of reaction potential is further illustrated by the wording of Postulate V, part B (pp. 38-99). The  $sE_R$  values computed by Yamaguchi from his extinction data are graphed (Fig. 8, p. 36), and equation 7 is fitted to them. While the curve as drawn passes through a maximum, the trend of the values seems to provide as good a basis for fitting a curve rising toward an asymptote. The fact that the plotted curve fits the data closely means little, since its equation has as many constants as there are computed values. (On the basis of equation 7, Hull follows Yamaguchi's lead in adding to the effects of food deprivation an inanition component  $\epsilon$

whose influence upon  $sE_R$  opposes that produced by the usual drive component. However, in view of the previous remarks, the assumption of the new factor seems quite premature.)

Postulate IX provides a summary of the relationship of  $sE_R$  to the complete set of intervening variables. The multiplicative relation presented in equation 19 clearly implies that  $sE_R$  is a linear function of each variable whenever the rest of the variable set is held constant. Postulate IX raises a problem concerning the balancing of units in equation 19. Presumably each of the quantities on the right side of the equation has its own unit. In *Essentials* only the  $sH_R$  unit, the hab, is specified. The *Principles* contained a definition of the mote as the unit of primary drive, but this unit seems to have been discarded in the current version, since there is no reference to it. Whatever the units may be for  $J$ ,  $K$ ,  $V_2$ , and  $D$ , no explanation is offered as to how their multiplication would make them compatible with the wat, the unit of  $sE_R$ , which, as defined, is essentially a variability unit.

Several features of the book and the system as a whole call for mention. First, Hull's single-minded preoccupation with quantification often leads him astray. Thus, empirical equations frequently contain almost as many constants as there are datum points fitted; certain arbitrarily chosen types of units, scales, functions, and quantification procedures are regarded as "natural" or "true" in some unexplained sense; a specious argument is advanced for the equality of asymptotes on the basis of the unit value assigned to them for scaling purposes; values obtained from human experiments are inserted into equations developed from rat data; a unit like the "wat" is quantitatively defined, but uselessly so because no rules of transformation are provided to make the unit applicable outside its defining context; certain equations, though quantitative in form, actually express only nonquantitative notions since no computations are possible, and the equations cannot be solved.

Secondly, *aficionados* of the postulate method in psychology will continue to be dissatisfied with Hull's newest exposition and exemplification of it. Terms like postulate, primary law, hypothesis, empirical generalization are equated; and, similarly, no distinction is drawn among hypothetical construct, symbolic construct, intervening variable, quantitative unobservable, and quantitative theoretical construct. As to the postulates, some are indeterminate in the form in which they are presented, while others are simply empirical generalizations based on specific numerical values of parameters employed in particular experiments. Many so-called corollaries are not deducible from their presumed parent postulates, but often represent either separate generalizations from other empirical data, or the germs of some genuine postulates. Basic functions in the system are not rational; one cannot say why, in different cases, the author chose a multiplicative function, or an expo-



nential one, or any other. In fact, it seems clear that the structure, methodology, and content of Hull's system are mixed, and that it as often assumes the descriptive-inductive character he ostensibly renounces, as the hypothetico-deductive character he espouses. He has merely mistaken quantification for deductive theory.

There are a number of miscellaneous ways in which the book and the systematic presentation are marred. A few illustrations will suffice. In various places, the author states, with a sort of taxonomic abandon, that his behavioral theory applies to mammals, vertebrates, or just plain organisms. Terms appear in the glossary which are not found in the text, and vice versa; also, the glossary meticulously records among other things that  $f$  = function of ( ),  $\log$  = logarithm, and,  $\sigma$  = the standard deviation. Interpretive or theoretical statements are occasionally written as if they were data statements (e.g., pp. 23, 73). Part C of Postulate XIII is thrown in only because, as the author says, he feels he "must add" it. Graphs of stimulus generalization gradients still follow the dubious practice of using j.n.d. units. In corollary IX, p. 84, work is assigned gm, instead of gm-cm, units. At one point (p. 34) the number of responses to produce extinction ( $n$ ) is used to approximate  $sE_R$ , though the relationship between the two variables is later specified to be nonlinear (pp. 111-113). Occasionally, the author makes an observation or argument or rationalization which may not only be irrelevant to his topic in hand, but also is incorrect or uninterpretable; such, for instance, is his remark that, in the  $sE_R$  quantification technique based on Thurstone's Case III the investigator makes comparisons "presumably without error"—a notion which has no meaning outside a theory of the human observer as a measuring machine. In developing his system, the author relies on data from a small number of experiments without indicating his criteria for selecting them; moreover, some types of data already in the literature are ignored although they bear on the system in a contradictory way, examples of this being the data on pre-conditioning or operant level response strength, and the data of Strassburger on the effects of varying drive level during acquisition.

In the mathematical treatment, also, we find some minor misdirections. Thus, the numbering of equations breaks down at times, as when equation 14 (p. 52) acquires a new number later in the text (equation 45, p. 107). In the sample computations performed by the author the usual rules for significant figures are not followed. On page 37, the constants of equation 7, all four- or five-digit numbers, give an impression of accuracy far beyond the level warranted by the kind of data from which the equation was derived. Equation 16 (p. 56) is badly written, since it should read,  $sE_R = f(J) = 10^{-.163J}$ . On page 102, an  $sE_R$  increment of .426 is added to the coefficient of an  $sE_R = f(N)$  equation rather than to any one of the other constants, for no clearly valid reason. On page 53, a multiplication by 2 is carried out for arbi-



trary reasons, and then only on one side of the equation. On p. 81,  $n$  is set equal to two different expressions in equations 33 and 34. Then the right side of equation 34 is substituted for one constant in equation 33. Typographical errors are rare in the book: one case is found on page 94 where the equation 25 referred to deals with  $K$  and not  $V$ .

Despite the brevity of this little book, it unmistakably carries Hull's personal seal and flavor. On occasion, he steps outside the limits of presenting his theory; here, to expound on scientific method; there, to exhort his reader to the "fearless performance of critical experiments"; and elsewhere, to give us his own chronology of growth as a behavior theorist, "through twenty-five volumes of hand-written notebooks," beginning in 1915 (or three years before he received his doctorate). Stylistically, too, the prose identifies itself as Hull's familiar hand.

Hull's theoretical work has always been marked by an effort to achieve explicitness and testability, because he believed those virtues to be essential to progress in theory construction. They also make criticism easier, as Hull was well aware, but he properly regarded that as a requisite of progress. The *Essentials* was his attempt to improve on the *Principles*, perhaps only in a stopgap way, since in it he declared his intention soon to publish a major work. This last project has now been cut short by his untimely death. Even his critics will miss that next book.

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PARSONS, TALCOTT, & SHILS, EDWARD A. (Eds.) *Toward a general theory of action*. Cambridge: Harvard Univer. Press, 1951. Pp. xi+506. \$7.50.

This volume is the result of a project, subsidized by the Carnegie Corporation and the Harvard Department of Social Relations, designed to assess "the theoretical foundations underlying the synthesis which had been worked out on the organizational level through the foundation of the Department of Social Relations." As such, it becomes an important test case of the fruitfulness of the "interdisciplinary" attack on problems of social science theory. Since the furious waving of the interdisciplinary banner in recent years has mobilized many recruits and strong institutional support, the volume deserves careful study, and certainly far more detailed analysis than can be given it in this brief review.

The project staff consisted of two sociologists (Talcott Parsons and Edward A. Shils), a psychologist (Edward C. Tolman), and a social anthropologist (Richard C. Sheldon). During the five-month period of the project, the staff operated both as an independent unit and in conjunction with two secondary echelons—one, a discussion group including G. W. Allport, C. Kluckhohn, H. A.

Murray, R. R. Sears, and S. A. Stouffer, and the other, a larger discussion group including "all interested members of the Department of Social Relations." Funneling from this pool of interdisciplinary effort into the present book is a brief general statement on "Some Fundamental Categories of the Theory of Action," drafted by Parsons and signed by all contributors; a lengthy elaboration of the theory in the form of an essay on "Values, Motives, and Systems of Action" by Parsons and Shils (with the assistance of James Olds); the specification of "A Psychological Model" by Tolman; and a group of briefer contributions on "applications" of the theory of action by the five secondary participants named above. Sheldon contributes a short methodological section, "Some Observations on Theory in the Social Sciences." In violation of the spirit of interdisciplinary fair-play, it may be revealed that the formulations of Parsons and Shils dominate the book both in the allotment of space and conceptually. Accordingly, they must dominate this review.

The "theory of action" is mainly a set of formulations which talk about the variables of a *projected* theory. There is also, as is becoming inevitable in social science, much talk about the talk about the variables of this projected theory. Beginning with this second level of talk (the "meta-meta-language?"), we itemize a few of the authors' conceptions about the objectives and properties of the theory of action:

1. The aim is to "develop a unified conceptual scheme for theory and research in the social sciences."

2. In the execution of this task, the authors propose to do justice to the "whole course of development of work in the social sciences" (p. 239). In particular, they claim responsiveness to the emphases of Freud, Allport, and the study of animal behavior (as mediated by Tolman) in psychology; in sociology, to Durkheim, Weber and, to a lesser extent, Park and Thomas; in anthropology, to Boas, Kroeber, Sapir, and others.

3. The *immediate* intention is to identify a set of variables adequate to the "descriptive analysis" of the state of "systems of action" at a given moment. However, these variables are to be chosen in such a way as to permit (ultimately) "dynamic analysis" of the "changes of the system through time," in terms of changes in the values of the variables.

4. The authors distinguish four "levels" of scientific systematization: "(1) *ad hoc* classificatory systems, (2) categorical systems, (3) theoretical systems and (4) empirical-theoretical systems" (p. 56). The "categorical type . . . involves a system of classes which is formed to fit the subject matter, so that there are intrinsic relations among the classes, and these are in accord with the relations among the items of the subject matter" (p. 50). A "theoretical system is a categorical system whose laws relating elements [i.e., the categorical variables or "classes"] have been formulated." Theoretical systems generate deductive consequences which "make . . . predictions about . . . how empirical systems . . . behave if an ideal set of scientific or 'standard' conditions were to exist" (p. 51). "Empirical-theoretical systems . . . the long term goal of scientific endeavor" . . . are capable of "predicting changes in empirical systems outside special experimental conditions" (p. 51). Parsons and Shils locate the present status of the theory of action in the *categorical* domain.

5. Nevertheless, despite the fact that their categorical system is admittedly "free of laws which relate its elements," the authors claim, at many points, to be using *deductive* techniques, and, in general, feel that they "carry deductive procedures further than is common in the social sciences" (p. 49).

Turning now to the talk about the variables of the proposed theory of action, we face a formidable problem. So fully do the "categories" of the theory reflect the complexities of its subject matter, that a meaningful précis is impossible. Indeed the some 250 closely printed pages devoted to the theory is itself a précis, consisting of a relentless barrage of definitions and classificatory elaborations of these definitions, but devoid either of empirical application or illustrative explication of the defined variables. Perhaps the following paragraphs will impart the flavor, if not the sense, of the formulations.

The theory of action makes statements about the action of individual *actors* or collectivities of actors. The descriptive unit for the analysis of action is the *orientation of action*. An orientation of action "involves a set of *objects of orientation*," characteristic of the *situation*. The two principal classes of objects provided by the situation are: "(1) nonsocial, that is, physical objects or accumulated cultural resources, or (2) social objects, that is, individual actors and collectivities" (p. 5). "A specific combination of selections relative to . . . objects," made from the available possibilities of selection in a specific situation, defines an *orientation of action* in a given case. The orientation of action may be broken down into two analytically independent sets of categories: *motivational orientation*, and *value-orientation*. "Motivational orientation refers to those aspects of the actor's orientation . . . which are related to actual or potential gratification of the actor's need-dispositions" (p. 58) and is characterized by three *modes*: *cognitive*, *cathectic*, and *evaluative*. "The *cognitive* mode involves the various processes by which an actor *sees* an object in relation to his system of need-dispositions." "The *cathectic* mode involves the various processes by which an actor invests an object with affective significance." "The *evaluative* mode involves the various processes by which an actor allocates his energy among the various actions with respect to various cathected objects in an attempt to optimize gratification." "*Value-orientation* refers to those aspects of the actor's orientation which commit him to the observance of certain norms, standards, criteria of selection, whenever he is in a contingent situation which allows . . . him to make a choice" (p. 59). Such norms or standards exist as *need-dispositions* within the actors who observe these rules. When abstracted from the concrete value-orientations of actors, these rules form a set of cultural *value standards*. Three *modes* of value-orientation—*cognitive* (standards of validity), *appreciative* (standards of "immediate gratificatory significance," e.g., aesthetic), and *moral* (standards which assess the consequences of actions for the "integration" of personality, and the "integration of the social systems" in which the actor participates)—"parallel the modes of motivational orientation" (p. 60).

An "organized plurality of . . . orientations of action constitutes a system of action" (p. 5). The "organized system of the orientation and motivation of action of one individual actor" is the *personality system*. A *social system* is a

system of the "interactions of a plurality of individual actors" (p. 7). *Cultural systems* are not, strictly speaking, systems of *action*. Culture exists "as a body of artifacts and as systems of symbols," and is embodied in the "orientation systems of concrete actors" (p. 7). The variables for the description of personality systems derive from various "fundamentals of behavior psychology," from psychoanalysis and other sources, and are brought to converge on the concept of *need-dispositions* as the basic organizational unit. *Role* is the unit of the social system, while cultural systems are *systems of ideas, systems of expressive symbols*, and systems of *standards of value-orientation*. Despite the existence of complex interrelations, and a homology of certain of the concepts at the various levels, social systems are not reducible to personality systems, nor are cultural systems reducible to any combination of the variables of either. A major concern of the theory of action is to locate certain "points of articulation" among the three types of "systems," a pivotal such area being the concept of *role*, where "value pattern, social structure, and personality come together" (p. 243).

Wherever one moves in the theory of action, one stumbles into a set of five classificatory dichotomies called "the pattern variables," to wit: affectivity—affective neutrality; self-orientation—collectivity-orientation; universalism—particularism; ascription—achievement; specificity—diffuseness. The "pattern variables" had originally been created by Parsons for the classification of certain structural components of *social* systems. In the over-all theory of action, it happily turns out that the pattern variables are capable of momentous feats of integration at every level. They serve as the basis for characterizing the actor's concrete *value-orientations*. They lead, via 32 cross classifications, to the cataloging of *need-dispositions*. Similarly, they lead, on the level of social system, to a classification of *role-expectations*. They enter also "on the cultural level as aspects of value standards" (p. 78).

The mode of presentation of the theory of action imposes severe difficulties on any attempt to characterize its methodological status. Its authors write as if fighting a continuous but losing battle to express the ineffable. The exposition consists of a series of definitions in abstract, discursive verbiage. Each concept is defined not once but in many different contexts throughout the discussion, so that a given term acquires a complex and slippery array of meanings by a process of cumulative accretion. It is hard to find a place for many of the definitional statements within the logic of definition. The authors' belief that they are using *deductive* procedures seems to spring, in part, from the intention to construct explicit definitions from the so-called "elements" (primitive terms) of the "frame of reference of the theory of action." However, these "elements" or primitives—which strictly speaking require operational and perhaps implicit definition, but may not be explicitly defined—are given explicit definitions, often in terms of the later, presumably derivative, concepts.

The authors show laudable modesty in classifying the "theory of action" as a "categorical system." Nevertheless, it remains impossible to evaluate the potential *theoretical* fruitfulness of the variables ("cate-



gories") put forward. All that the "system" contains is an extended set of explicit definitions. Operational (empirical) definitions are not given for a single one of the many concepts, nor are there clues in the discussion as to how and from what sources such definitions might be constructed. Sheldon, in his contribution on the methodology of social science theory, notes this embarrassing circumstance, but derives much solace from the belief that theoretical concepts (and principles) are "free creations of the human intellect." Whatever the virtues of Poincaréan "conventionalism," it always remains desirable to ask, "how free?" And the wisdom of constructing an intricate array of theoretical concepts independent of step-by-step efforts, however tentative, to tie these concepts to empirical states of affairs may be seriously doubted. The theory of action does not even permit the determination of *where* operational definitions are relevant, because few grounds are available for deciding which of the "categories" correspond to systematic independent variables, which to intervening variables, and which to dependent variables.

Tolman's section on "A Psychological Model" marks one of the frequent new phases in his thinking. Tolman now feels that all intervening variables used in psychological theory are defined *both* functionally, and by the attribution of "postulated, ostensive [i.e., "existential"] properties." The referents may be "neurophysiological," "phenomenological," or the components of "*a sui generis model*." Tolman's new theory develops such a model "by various analogies from simple physics and mechanics, from Lewin's 'topological and vector' psychology, and from common experience" (p. 283). The attempt to do justice to certain of the conceptual demands of the theory of action unleashes the Lewin in Tolman to a greater extent than ever before. Indeed, the result is very much like a reshuffling of Lewin into a forced independent-intervening-dependent variable schema, with the addition of a few special twists to accommodate some of the requirements of the theory of action—all this decorated with many Tolmanesque flourishes.

Tolman's model relates environmental, "drive arousal," and individual difference, independent variables to a complex set of intervening constructions. The major intervening variable units are: (a) a *need system* (Lewin's treatment of "need structure" ripped out of the life-space and massaged a bit by Tolman), (b) a *belief-value matrix* (the repository for Parson's "need-dispositions" and Tolman's former "means-end-readinesses"), (c) an *immediate behavior space* (Lewin's momentary life-space as modified to contain Tolman's "expectations") and (d) a *restructured behavior space* (the "immediate" space as "restructured" by "locomotions," "learning," or the "psychodynamic mechanisms"). The postulated interrelations among these "variables" (and the subvariables constitutive of each) are of the customary programmatic or "need to know" sort. At many points, the reliance on Lewin is more a matter of form than of content, for many of the apparently Lewinian concepts (e.g., "distance," "direction," "locomotion") are homonyms used in quite a different sense. The model is also



endowed with "'electromagnetic' charges" as needed, and is often the victim of such incredibly ad hoc calumnies as: "The *strength* of a field force is . . . directly proportional to the product of the need-push and the determining valence . . . and inversely proportional to the square of the behavior-space distance between the region of the behaving self . . . and the region of the corresponding valence" (p. 340).

The model is indeed *sui generis*. Tolman, however, berates the model with his usual candor, often implying that it may well prove necessary to do away with the thing. Should this be the model's unhappy fate, the field will nevertheless recall with nostalgia a fetching feature of its conceptual physiognomy called "the nose of the behaving self."

Of the five "applications" of the theory of action which comprise the final portion of the book, only two are related to the theory in any determinate sense. One of these, Kluckhohn's "Values and Value-Orientations in the Theory of Action," is not an *application*, but a further definitional analysis of this topic. The other, "An Empirical Study of Technical Problems in Analysis of Role Obligation" by Stouffer, concludes with this warning: "one of the most important values of this paper should be its service as a brake on the enthusiasm of those who may anticipate quick and easy progress in moving from highly abstract concepts of social science to empirical operations" (p. 494). Murray's essay on the classification of needs, and Sears' paper on the desirability of dealing with individual and social behavior within a single theory, are both relevant to the theory of action in some broad sense, but do not proceed within the "categories" of the theory. Perhaps the most astounding property of the theory of action is its capacity to "postdict," as an *application*, Allport's 1950 SPSSI address on "Prejudice."

At a time when the route towards, *even the possibility of*, comprehensive theory in each of the individual social sciences is in doubt, it is more than sanguine to begin the construction of a unified theory for *all* social science. The "theory of action," in its present form, gives substantial support to this statement. Yet, it is possible to argue that certain of the limitations of attempts at theory in the individual social sciences may derive from failure to explore concrete interdisciplinary relations among emerging concepts and empirical findings. Psychological theorists who try to deal with complex aspects of human behavior have been too long content to talk in such diffuse terms as "the role of *social or cultural factors*" within the individual behavior phenomena that they analyze, or "the effects of the *social environment*." It is possible that each one of the social sciences could benefit from modest attempts towards the coordination of developing empirical knowledge, and mutual accommodations among concepts. Embedded in the ponderous formulations of the "theory of action" are evidences of the possible fruitfulness of interdisciplinary effort, at this less pretentious level. The consideration of "points of articulation" among psychological, social,

and cultural "systems" raises, at least obliquely, some interesting problems; the importance of making provisions, within approaches to psychological theory designed to cope with complex behavior, for variables adequate to the *empirical phenomena* designated by such terms as "the complementarity of expectations," "role-expectations," etc. is plausibly, if elliptically, argued. If the participants had kept more firmly in mind the "Towards" in "Towards a General Theory of Action," and had been less committed to the "General," and to the singular form of the autistic noun, "Theory"—the result might have been a useful preliminary mapping of common and differential problems of social science methodology.

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TINBERGEN, N. *The study of instinct*. London: Oxford Univer. Press, 1951. Pp. xii+228. \$7.00.

It has sometimes happened in the history of science that a book has had influence greatly out of proportion to its apparent intrinsic importance. Paley's *Natural Theology*, permissible Sunday reading in a straight-laced time, influenced a whole generation of future English biologists by combining a wealth of natural history with its polemical "evidences of the existence and attributes of the Deity, collected from the appearances of Nature." Tinbergen's book may well serve a similar purpose for psychology. It comes as a partial answer to Beach's recent observations on the need for gathering behavioral data on a somewhat more representative number of species than American psychologists appear at present to be studying. *The Study of Instinct* should provide a means whereby the American student might become better acquainted with the increasing number of European studies of animal behavior and with the somewhat different approach to common problems which the zoologically-oriented ethologists are developing.

To those who have been reading the postwar journals *Behaviour* and the *Bulletin of Animal Behaviour*, or who have seen the *Fourth Symposium of the Society for Experimental Biology*, the general conceptual organization of Tinbergen's book will come as no surprise. Those who have confined their recent reading on animal behavior to the *Journal of Comparative and Physiological Psychology* and other American journals will find much that is different in concepts, in methods, and in factual description. Because the word "instinct" has acquired some of the connotations associated with "teleology," "reification," "purpose," and other such residues of the valiant battles of the Watsonian twenties, there is a real danger that the very people who can do most with Tinbergen's contributions will avoid them. It is hoped that this review will serve as some assurance that a book with "instinct" in the title can be at once worth while and exciting to the biologically-minded psychologist.

Tinbergen at the outset explicitly avoids the two traps which await every student of innate behavior. He refuses to accept the directiveness of Russell or the purposiveness of McDougall as explanation. While, as he says, the fact is undeniable, what the ethologist must study are the causal conditions of purposive behavior. He also rejects the subjective approach of Bierens de Haan that "an animal hunts because it is hungry," observing that this is "a guess about the possible nature of the animal's subjective state." What is needed is a "physiology of behaviour" that utilizes the objective methods of the physiologist but applies them to the problem of behavior rather than to the processes of organ systems as such. All of this is not, of course, startling, since any American physiological psychologist would probably say about the same things. However, Tinbergen next notes that the psychologist in America, although objective in methodology, has specialized on the higher types of behavior. Because of his interest in human conduct and his acceptance of man's evolutionary descent, he has become preoccupied with the *pre-human* behavior of mammals. His neglect of innate behavior, Tinbergen feels, "is due to the fact that it is not generally understood that learning and many other higher processes are secondary modifications of innate mechanisms, and that therefore a study of learning processes has to be preceded by a study of the innate foundations of behaviour."

Concerning methodology Tinbergen emphasizes "the importance of a complete inventory of the behaviour patterns of a species." Specialization on limited problems without knowing the behavior repertoire of the animal studied can lead to unwarranted generalizations, as can use of a few specialized techniques for all problems. "In view of the differences between any one species and another, the only thing that can be said for certain is that one should *not* use identical experimental techniques to compare two species, because they would almost certainly not be the same to *them*."

The major portion of the book is devoted to a survey of the roles of external stimulation and internal conditions responsible for innate behavior. The evidence is chosen from a wide variety of species and the insufficiency of merely determining the limits of sensitivity of receptors is emphasized, as follows:

The animal's own world is not only dependent on what its sense organs can or cannot receive. Its sensory world is still more restricted; it is composed of sign stimuli, at least as long as we are dealing with innate responses. . . . The strict dependence of an innate reaction on a certain set of sign stimuli leads to the conclusion that there must be a special neuro-sensory mechanism that releases the reaction and is responsible for its selective susceptibility to such a very special combination of sign stimuli.

This process Tinbergen calls the Innate Releasing Mechanism. The extent to which this IRM is influenced by learning is looked upon as a matter for future research. Here Tinbergen might have profited by a

more extensive search of the American literature on the "pre-human" behavior of such mammals as the rat, where good evidence in abundance may be found for a wide variety of acquired alterations of both stimulus pattern and IRM, particularly in such "innate" states as hunger and thirst.

His attempt at synthesis of the relevant data is along the general lines made familiar by Morgan's and Beach's concepts of internal mechanisms of motivation, coupled with a concept of hierarchical organization of the neural structures underlying innate behavior. As in all modern attempts to understand the physiology of need states, the endocrine system and hypothalamic nuclei are pressed into service. This material is presented with all modesty as a very tentative way of helping to "put our thoughts in order."

The role of learning in determining the precise sequence of these behaviors is again minimized, although other sections of the book present evidence to show that a great deal of individual modification of innate patterns does take place. Worthy of attention by American students of learning is Tinbergen's survey of what he calls the "innate disposition to learn." Many species show rapid and complex learning of some problems and almost complete stereotypy in other problems seemingly no more difficult. The mechanisms of naturalistic learning deserve more investigation.

It is difficult to make a critical evaluation of *The Study of Instinct* without retracing all of the polemical pathways of the 1920's. But it should be recognized that the investigation and the thinking of the European ethologists are far more rigorous than that of the field naturalists of the 19th century. Their concept of instinct, although similar to that of McDougall, is much better rooted in controlled observation. The final impression one gets from Tinbergen's book is that of the immense richness and incredible variety of behavioral phenomena which are there for the studying if we break out of our rat-monkey-man triangle. Tinbergen has no neat and tidy system and his problems sprawl over a range so wide as to appear to classify all behavior as "innate." But the facts he presents must be assimilated if the comparative method is to be something more than a misleading conventional symbol in American psychology.

L. I. O'KELLY.

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CAMERON, NORMAN, & MAGARET, ANN. *Behavior pathology*. New York: Houghton Mifflin, 1951. Pp. xvi+645. \$5.00.

This book is a welcome departure from the great majority of texts in abnormal psychology in that the authors diligently attempt to present clinical material in terms of the interrelationships of developmental, learning, and biosocial factors. Whereas many authors give



only lip service to the importance of these factors—their texts falling quickly into the Kraepelinean tradition despite prefatory statements to the contrary—Cameron and Magaret never seem to lose sight of the fact that these factors are always operative. The following statement by the authors indicates their approach as well as the scope of the book:

We begin where birth begins, with the unmistakable needs of the neonate, and the succession of inescapable frustrations to which he is exposed because of his helplessness and the complexity of the social milieu. This points up the inevitability of conflict and anxiety; and it clears the way for our recognition that all persons must acquire techniques for overcoming anxiety or at least making it tolerable. The acquisition of such techniques and their modification are both examples of social learning; and this holds good, of course, for the acquisition and modification of pathological techniques, as well as normal ones, and for therapy as well as pathogenesis.

That this book is seriously concerned with general principles and constructs may be seen from the following topic headings: learning in a social situation, learning and temporal sequences of behavior, selective learning, language behavior, socially organized thinking, social role-taking.

It is perhaps unfair to have compared this book to other texts in abnormal psychology, most of which are adequate for a single course. This reviewer agrees with Carmichael's introductory statement: "... a possible value of this book would be lost if it were to become merely a useful professional guide to those who already have wide expert knowledge of man's normal and abnormal behavior. Actually the present volume is so clearly written and its scientific and yet eminently sensible point of view is so effectively presented that it is hard to believe that any educated man or woman who reads it can fail to acquire a new kind of understanding of why normal as well as abnormal people act as they do."

SEYMOUR B. SARASON.

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SCHNEIDERS, ALEXANDER A. *Introductory psychology: The principles of human adjustment*. New York: Rinehart, 1951. Pp. xv+461. \$4.00.

This book is comprehensive enough to give the student sufficient knowledge of the field, yet brief enough to be compressed into the standard three-hour one-semester course. The topics are organized around two fundamental psychological concepts, personality and adjustment. The first two chapters adequately introduce the student to psychology, first by discussion of what psychology is not, second by a systematic analysis of what psychology is. The latter includes the historical background and meaning of the term; representative viewpoints in modern psychology, including structuralism, functionalism and psycho-



analysis, hormic psychology, behaviorism, and Gestalt psychology. Consideration is then given to the methods and techniques of psychology, including subject matter and methodology, methods of observation, psychological techniques, measurement, statistical analysis and inductive generalization.

Following this comprehensive introduction, the author has carefully organized the material into four sections dealing with (a) personality and adjustment, (b) mental activity and experience, (c) human motivation and behavior, and (d) the application of psychology.

In his presentation of the material the author is more explanatory than argumentative. Though source material has only been infrequently indicated, the material is presented in line with empirically established facts or generally accepted hypotheses. Where facts or hypotheses are inadequate, the author presents his own concepts and interpretations, always however, in the light of whatever empirical findings are at present available.

A glossary defines all unfamiliar terms appearing in the book. A list of selected readings at the end of each chapter provides the student with further reading. Without violating logical or psychological principles, the author has left room for the flexibility of teaching. The many facets of psychology are so integrated as to relate each section of the book clearly to the other sections.

To assist the student, the author has prepared a workbook consisting of objective test exercises, topics and questions for discussion, and suggested projects to accompany each unit.

This reviewer heartily recommends the book without out any reservation as a brief, clear, and concise text in introductory psychology.

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HOFSTÄTTER, PETER R. *Die Psychologie und das Leben*. Wien-Stuttgart: Humboldt-Verlag, 1951. Pp. 287. Univ. Bd. 23.

Reading *Die Psychologie und das Leben* leads naturally to a consideration of a contrast in cultures. For here is a book similar in title to those published in America by such psychologists as Ruch, Josey, Langer, Powers, McConnell, *et al.*; yet the material presented is surprisingly dissimilar. This book serves as an indication that the term "psychology" may, in different parts of the world, have widely divergent meanings, with the training for "psychologists" apparently reflecting such differences. Much of what American and German psychologists used to share in common has apparently been lost—if this book is truly representative of present-day German psychology.

Judged by our own standards, the book has very little to do with psychology and even less to do with psychology *and life*. It is devoted instead to psychologizing and to a philosophical concern with the

essence of being, the latter reflected in such questions as: "Und was bin ich eigentlich wirklich?" Such concern provides much of the context within which psychologizing occurs, with the material presented stemming not from operational, scientific, experimental, or empirical procedures, but from literary and philosophical sources. Included, however, are abbreviated summaries of the psychologies of such persons as Hull, Lewin, Pavlov, Skinner, and Tolman. It is surprising to find no consideration of the work of such compatriots of the author as Freud, Jung, and Adler.

Psychology appears to be regarded by Hofstätter less as a science than as a culturally bound and determined expression of the relationships between various modes of looking at the world and the "immediately knowable." Offered as the five main modes of approach are (a) the metric, involving the relationships of physical and chemical properties to psychological perception; (b) the Gestalt, involving the relationships between biological "wholes" and psychological aspects of behavior; (c) the noetic, involving the Geisteswissenschaften and their relationships to psychological values; (d) the sociological, involving the relationships between social sciences and psycho-social expectations; and (e) the artistic, involving the approach perhaps closest to the "immediately knowable."

In summary, the author seems more concerned with psychologizing about a miscellaneous range of topics rather than with psychology as a science; with opinions rather than with facts; with literary and philosophical speculation rather than scientific analysis; with theory rather than data; with the raising of problems—literary, philosophical, political, psychological—rather than with the solving of them.

Nevertheless, Hofstätter presents an erudite book that has a stimulating breadth of coverage. For those who find the language no barrier, it will provide enjoyable reading, regardless of the fact that the material happens to have little to do with "psychology and life."

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DEWEY, RICHARD, & HUMBER, W. J. *The development of human behavior.* New York: Macmillan, 1951. Pp. xv+762. \$5.50.

After reading the statement of purpose in the preface, this reviewer turned to the table of contents with some skepticism. So many attempts to achieve the grand synthesis in the social sciences have proved unsatisfying. He was pleasantly surprised to find a social psychology text that was both sociological and psychological, but he regretted to find the title misleading. The text does not adopt human growth and development as its frame of reference.

The first part of the book attempts to bring together the major facts of (a) biological heritage, (b) environment, and (c) personality.

Not all psychologists will want to treat "personality" as an independent entity, on equal footing with the biological heritage and environment. Some will prefer to regard it as the behavior outcome of a modifiable organism with a background of inherited characters in interaction with the environment.

The second part of the book is given over to the social psychology of age groups. However, the chapters given to infancy, childhood, adolescence, and the adult years and old age fail to reflect either the continuity of events which make up personality growth and development, or the effect upon such growth of the many social and social-psychological forces. True, the authors mention a few important developmental principles, such as "the organism develops as a unit," "maturation proceeds by coordination of two rather distinct processes, differentiation and integration," and "maturation is not uniform for all children." However, the specific researches which have given rise to these important concepts are almost completely neglected. For example, no material from developmental embryology appears. Shirley's important work is mentioned primarily to illustrate the fact that infants have little social interest in other infants.

In several places the concept of an emerging self is identified as a succession of social roles, one after the other. The authors acknowledge their intellectual debt to Cooley and Mead, who have undoubtedly afforded an important theoretical basis for a concept of the self. A philosophical position taken several decades ago, however important then in conceptualizing an undefined area, is hardly a substitute for the important empirical material which now undergirds a somewhat different concept of the emerging self. These authors make no mention of the important researches on the contribution of language to the development of the self concept.

In the discussion of adolescence, too, this reviewer is concerned that some of the most important papers on growth and adaptation during the adolescent years, such as those by Shuttleworth and by Jones, are missing from the references. What has been done is sound in a broad descriptive way. So much very important developmental material has, however, been omitted that the reader will gain no comprehension of the continuity of the process of growth.

Part III, much longer than Part II, deals with abnormal status, deviant roles, and personality. This reviewer believes the discussion of role status and the social psychology of minority status is one of the most comprehensive he has seen. Prejudice as a two-way process is excellently presented. The problem of group relationships is more objectively considered than in many current discussions which assume the majority to be "wrong" and the minority "right" when conflicts arise.

The final part has to do with social institutions and personality.

Here the school, religion, occupational status, the economic system, and the social psychology of mob behavior are considered. In a concluding chapter, the authors tackle the dimensions of the good society. Acknowledging the relativity of values, they define the good life in terms of the "golden mean of behavior" and venture to set up certain characteristics of a good society, although they recognize that good societies may take many forms.

This reviewer's criticisms thus far primarily reflect his expectation that the text would be developmental in its orientation. Viewing it as a contribution to social science, or more specifically to social psychology, he considers it a significant attempt to organize data concerning human behavior simultaneously from two traditionally different viewpoints. If the authors have been encyclopedic rather than critical in assembling and selecting their material, their attempt at synthesis should be viewed with some charity, for there is little intellectual precedent for their work.

One may rightly wonder where this book will be of greatest use. This reviewer believes that the typical undergraduate student in social psychology may find the book confusing. The intelligent undergraduate who has a broad background of reading in both sociology and psychology may find it stimulating. Graduate students with strong intellectual drives toward synthesizing concepts from many areas will also find it of interest. Psychological studies of concept formation affirm that adequate generalizations are made only in the presence of a rather large background of specific facts, data, ideas, and concepts. Hence a book which attempts to achieve the grand synthesis may properly be read after much specific course experience in sociology and psychology. Probably it should not be offered as a general text for the beginning student. The authors have made a good beginning toward bringing together and interpreting facts concerning human social behavior. The insights they develop reflect their own years of study; their product may not lead the sophomore who uses this as his social science text to a similar understanding.

DALE B. HARRIS.

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KUHLEN, RAYMOND G. *The psychology of adolescent development*. New York: Harper, 1952. Pp. xvii+675. \$5.00.

In general, this textbook is solidly grounded in research and stresses practical applications. It includes critical sections, simply and briefly stated, with reference to most of the research quoted. By and large, theory is skirted. Psychoanalysis, for instance, is listed with three page entries in the subject index. The first two of these are brief and nonevaluative. No direct mention of psychoanalysis was found on the third page cited in the index. Freud is not listed in the author index.



The point of view is an eclectic, learning-organismic approach. The book presents broad (the range of references per chapter is from 24 to 94) and frequently unusual coverage of material.

The author points out repeatedly that data gathered on adolescents do not possess their full value in the absence of comparable data for adults. He emphasizes and documents consistently throughout the text cultural determination, multiple causation, and the functional interrelatedness of all aspects of development. He frankly includes certain ideological, thought provoking, "value including" sections (e. g., in the chapters dealing with American culture and personality and with adjustment to organized society).

There is a knowledgeable, practical section on the consequences of shifting to a democratic educational procedure from earlier, more formalized experiences. Unusual data on school variability, teachers' relations with students and their improvement, and a research and experience based section on grouping to reduce student variability help to make up one of the best chapters in the text (Chapter 10, "Educational development of adolescents and adjustment to school").

Effective use is made of some of the vivid material gathered by Baldwin, Kalhorn, and Breese on young children, to illustrate the range of home relationships. Brief case histories are used throughout. Sociometric data are discussed very practically, although the matrix presentation of such data is not covered. There is a brief discussion of autobiographical and other written materials as methods of studying individuals, although the emphasis on dangers and ethical considerations to be made in the use of such methods is not strong enough. Similarly, the section on the interview makes it look very simple; but interview ethics, and the need for intensive, supervised training in interview techniques are touched on only implicitly.

While the author demonstrates that he is aware of the operational meaning of "intelligence" he slips occasionally into phrases which carry excess meaning, such as "sheer underlying intellectual potential" (p. 87).

In the last chapter the treatment of test construction seems to the reviewer sketchy. It could lead to an uncritical acceptance of paper and pencil personality and adjustment tests, and an underestimation of the dangers of them in the hands of amateurs.

It seems to the reviewer, however, that the merits of the book clearly outweigh its defects. Factors of research orientation, clear but critical writing, and the author's obviously wide and "well-digested" practical experience make this a very suitable text for advanced undergraduates and beginning graduate students in education and human development.

BOYD McCANDLESS.

*State University of Iowa.*



WATSON, ROBERT I. *The clinical method in psychology*. New York: Harper, 1951. Pp. xii+779. \$5.00.

Watson's volume represents an excellent attempt to perform an impossible task. To undertake a text on clinical methods in psychology, pitched at both the "advanced undergraduate and graduate" levels, at the very outset requires courage. Along the way it calls for clinical and scholarly acumen, breadth plus intensity of knowledge, painstaking attention to tedious detail accompanied by ability to hold the reader's interest. These virtues are all reflected abundantly in this book. But in the end such an effort will inevitably be greeted by reviewers' dissatisfactions.

From the foregoing comments it should require no great skill in the clinical method to infer that the present reviewer feels somewhat guilty about offering several serious objections to an exceptionally sound piece of work. One immediate source of confusion concerns the audience which Watson is trying to reach. In his carefully elaborated "Apology to the Reader" he states that some previous acquaintance with diagnosis and therapy is taken for granted. For the undergraduate student such an assumption is likely to be ungrounded. For the graduate student some of the materials are too elementary, others seem to fit well into a first-year testing course, and still others would contribute to advanced study in the area. This unevenness of level probably means that each instructor will have to use the book selectively in terms of his own particular requirements.

The problem of balanced treatment of content also arises. Included are whole chapters on the Vineland Social Maturity Scale, diagnostic infant testing, and the Rosenzweig P-F Study, yet no discussion of such topics as interest tests and the Rorschach. The latter is excused, along with a number of other instruments, on the grounds of space-saving and also a contemplated separate volume, which of course leaves us with at least a temporary void. But apart from questioning the author's decisions on what to include and omit, the equal allocation of space to the Vineland Scale and the diagnostic interview, for example, appears dubious. On the positive side, it should be noted that the integration of diagnostic with therapeutic procedures has been accomplished in a highly effective manner.

Throughout the book there is a delicate weaving of research data into the pattern of clinical methodology. However, a notable exception to this approach is found in the absence of any research evidence bearing on the diagnostic interview. The negative findings of the VA Assessment Project, conducted by Kelly and Fiske, would provide a sober counterpart to the optimistic statement that the distinctive contribution of the interview is that it allows "the development of a total picture of the patient." Incidentally, the Assessment study, by far the most systematic investigation of clinical methods, does not even receive

passing mention in the volume. Another overlooked study by Simos (*J. Pers.*, March 1950) would serve to temper the author's conclusions concerning the validity of the P-F test.

A text of this sort must draw its substance from the richness of the author's own knowledge and experience. Yet in a field as amorphous as clinical psychology, knowledge and experience cannot help but be contaminated by biases based on varying degrees of truth. Watson seems to have bent over backwards in trying to keep his own values out of the picture. But the danger is ever present that the subtle pseudopods of propaganda (most of which the reviewer is willing to endorse, by the way) may engulf the unsuspecting reader before he has a chance to think for himself. This dilemma is unavoidable. Occasionally more overt manifestations of the "projective hypothesis" creep into print, as in the author's defensive belaboring of the assertion that the clinician is really a scientist.

In summary, the book contains fine sections on diagnostic appraisal and psychotherapy. The latter occupies about a third of the total. Extensive bibliographies, including supplementary references from *Readings in the Clinical Method in Psychology*, edited by Watson, are also provided. There is some discrepancy between the front-flap quote that "liberal references to cases are supplied" and the author's own statement about the deliberate "relative paucity of case history material." In any event, the latter was not felt to be a deficiency. On the whole, it is easy to forecast that this book will be widely used, but somewhat more difficult to predict how often and for what reasons the clinical Sherlock will wish to consult Dr. Watson.

GERALD S. BLUM.

*University of Michigan.*

PASCAL, GERALD, & SUTTELL, B. J. *The Bender-Gestalt Test: Quantification and validity for adults.* New York: Grune & Stratton, 1951. Pp. xiii+274. \$6.60.

Presented here is a quantified system for scoring responses to the Bender-Gestalt Test. The authors found that in normally intelligent adults, free from brain damage, deviations in B-G performance are greatest in psychotics, less marked in neurotics, and smallest in normals. Data are presented which show the differences in performance of these three groups as well as the overlap.

For clinical practice and research, this book is enlightening insofar as it presents an attempt at quantification of the B-G, a detailed description of typical deviations, and numerous illustrations (including 96 complete records). The contributions of this work seem lessened, however, by the following objectionable features:

1. The B-G Test as applied by the present authors is, like most scales used

in clinical practice, an ordinal scale, with steps of unequal magnitude. This objection seems especially pertinent here because the items are differentially weighted.

2. Assuming that in adults without cortical damage, deviations in B-G performance are due to interpretative difficulties, rather than to perceptual or motor factors per se, the authors relate these difficulties to the subject's attitude toward the task. This attitude they relate, in turn, to the subject's attitude toward reality, which is believed to reflect his ego strength. The assumptions on which this new rationale is based are quite debatable.

3. In clinical practice, the B-G has been found to be sensitive to various cortical disturbances even of a mild nature. This new book contains little material that would aid the clinician in differentiating B-G deviations due to cortical deficit from those due to psychogenic disorders.

ELEANORE OCHS.

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RAPAPORT, DAVID. *Organization and pathology of thought*. New York: Columbia University Press, 1951. Pp. xviii+786. \$10.00.

This is an interesting, informative, and quite scholarly source book of foreign literature dealing with the thought process and its pathology. The selections embrace psychology, psychiatry, and psychoanalysis, all brought together in an interesting fashion. Using the central theme of the motivation of thought processes as distinct from static analysis of thought mechanics, Rapaport has skillfully developed a symphony understandable to the sophisticate in all three fields.

Much of the material in this volume appears in English for the first time since uniqueness of resource was one of the criteria by which the author judged the suitability of inclusion. However, some notable exceptions which the dictates of central purpose made necessary add much to the continuity of text. For example some of Freud's work, though not translated for the first time, simply had to be included to round out the material.

The papers are grouped under five not necessarily mutually exclusive headings.

1. *Directed Thinking*. This section includes that material normally presented in classical psychology. The author points out that the difference between "Lewin on the one hand, and Ach and Buehler on the other, is as great as that between either of them and Claparède or Piaget, who are closer to clinical thinking than to experiment." The decision to omit some of the more important Gestalt contributions is regretted by the reviewer, although the physical dimensions of the project would have made their inclusion difficult.

2. *Symbolism*. In the reviewer's opinion this is the most valuable section in the book. More attention is given to this topic than to the others, which seems fortunate since modern trends in psychodynamics call for an understanding of symbol formation in the thought process. Some of the data reported by Silberer, Schroetter, Roffenstein, and Nachmansohn are both ingeniously

derived and surprisingly convincing to an academic psychologist. These experiments bear careful study and broader inclusion in our clinical teaching.

3. *Motivation of Thinking*. Here are included certain materials from Freud's *Collected Papers*, even though they had already been published in English. These inclusions were necessary for the development of a definitive volume. Also in this section is a very interesting paper by Heinz Hartmann on "Ego Psychology and the Problem of Adaptation."

4. *Fantasy Thinking*. Some papers of Bleuler, Varendonck, and Kris are included in this section.

5. *Pathology of Thinking*. Some of Schilder's work, as well as Bleuler's classical paper on "The Basic Symptoms of Schizophrenia" are the major contributions in this section. Bleuler's treatise is interesting to read, although much of it may be disputable in the face of modern evidence.

The author recognizes some of the volume's possible shortcomings. He has willfully theorized where the lack of objective evidence might have made some hesitant to speak. He has omitted the classical associationist, conditioned reflex, and conditioned response contributions because: "(a) my bias that these learning theories contribute little to a theory of thinking; (b) they are readily available, and my purpose was to gather those contributions which have received little attention; (c) I wished to avoid polemics in my comments." With the first of these reasons many will take exception. The classical research on conditioning seems like an oasis in a desert of psychoanalytic speculation to most psychologists. The third reason sounds strange in a theoretical document such as this, since most of the material might certainly be the subject of polemics. Nonetheless the volume is excellent, well written, scholarly, and certainly a mature piece of bibliographic research.

ROBERT P. FISCHER.

*Marietta College.*

BRUNSWIK, EGON. *The conceptual framework of psychology*. Vol. I, No. 10 of International Encyclopedia of Unified Science. Univer. of Chicago Press, 1952. Pp. iv+102. \$2.00.

Physically this is a slim volume, hardly a book, rather, a monograph of barely a hundred pages. Intellectually this is the equivalent of three books or, to put it conservatively, of one well-sized book and two monographs of about one hundred pages each. Such condensation, to whatever extent it can be achieved at all, is no mean feat and, like every achievement, has its price. Brunswik pays in both style and vocabulary. His style is overly parenthetical and allusive, his vocabulary formidably polysyllabic. Frequent use is made of small print and, worst of all, of an overelaborate terminology. All this has marred my pleasure. It may be that I am overreacting because I am so vividly reminded of a shared background which, for all its excellence, did not



often excel in the art of saying things clearly and simply. But this will do for cavilling at what is, nevertheless, a distinguished piece of work by a distinguished author. So I turn to the three major themes, first to the two monographs and then to the book, that are here "encapsulated." (This is one of Brunswik's favorite words.)

There is, *first*, a reasoned statement of the logical positivists' analysis of psychology. Intermediate between epistemology and science proper in generality, this analysis belongs to what is now known as the philosophy of science. Since the *Encyclopedia* in which Brunswik's piece appears is an enterprise in this area, one might have expected him to devote all the space at his disposal to that theme. Had he followed this course, his statement of the position would probably have benefited. But I have no quarrel with it as it stands; at least, no quarrel that is not better confined to the epistemological seminar. The position expounded is also known as logical behaviorism or operationism. Brunswik's favorite name for it is objectivism, not unreasonably, since the term suggests the heart of the matter, namely, that psychology deals with overt behavior and nothing but overt behavior. However succinctly, he does give many of the explanations and qualifications this formula requires. And he also covers cursorily the other main tenets of the position by indicating the fallacies of the various methodological holisms and emergentisms, and by explaining how a "mechanistic" and "deterministic" psychology can deal adequately with purpose.

The *second* theme, or the second monograph the author might have written, is a piece of advocacy for a special line of research. Now Brunswik, who is a psychologist as well as a philosopher, is entitled to his opinions or hunches as to which kind of research is at the moment most promising. Yet, I cannot help feeling that the way in which he closely intertwines logical analysis with advocacy is most unfortunate. For, being merely a philosopher, I know only too well how hard it is to convince scientists that logical analysis properly conducted is genuinely neutral in that it does not and cannot furnish the grounds for such special pleading. As to Brunswik's preference, he wants all psychologists to look for laws that relate or, as he says, "focus on" two groups of variables, both "molar" and nonphysiological, the one "central," the other "distal." A need, for instance, is a central factor; test scores and perceptual "achievements" exemplify distal variables. Because of the nature and the multiplicity of the "mediating" mechanisms, laws of this kind are as a rule statistical or, as Brunswik puts it, "probabilistic." The early American school known as Functionalism was the first to investigate systematically relationships of this type. There is thus some historical justification for Brunswik's borrowing their name for his particular brand of psychology or, as I would rather say, his emphasis. For the rest, if I could permit myself the expression of a



taste within science, I would tend to agree with this preference. So, by the way, do the students of Hull and Spence. Brunswik, rather amazingly, does not see that. This leads me to my last point.

The abstract of a book that is, *thirdly*, embedded in these hundred pages is a structural history of systematic psychology since Wundt and Brentano. The main task of a pragmatic history, such as Boring's, is the exposition of ideas in their causal sequence. Structural history is a comparative analysis of ideas in their logical interdependence. It is, no doubt, a discipline as admirable as it is difficult, requiring both considerable erudition and a philosophical turn of mind, a combination none too common. On the basis of the evidence presented, it is safe to say that few, if any, are better qualified than Brunswik to undertake this arduous task. But even structural history is a dangerous weapon in the hands of a man with a cause. In a pattern much too neat to be quite as thorough and exhaustive of the material as it looks, everything is made to point and converge toward the emergence of the new "functionalism," almost as the prophets of the Old Testament were ex post facto made into witnesses of the New. To write history in this vein is another less admirable trait of the background I share with the author. There is even an untranslatable German word for it (*Geschichtsklitterung*). I shall give two examples of the violence here perpetrated for the sake of a pattern. The one tries to eliminate a competitor; the other claims a spurious ancestor. The competitor, if this be the right word, is the behavior theory of the Hullians. This theory, we are told, since it "stays within the framework of associationistic tradition," can only give "token recognition" to stimulus patterning through "casually superimposed principles." As it happens, these principles are the very heart of the theory. Also, we are told that it can only interrelate its "intervening variables among one another and with peripheral-proximal boundary conditions," thus "paying with molecular encapsulation" for the progress it achieves. What, pray, is either molecular or peripheral or proximal about a rat's hunger and the piece of Purina it covets? The spurious ancestor of the new "functionalism" is the act psychology of Brentano. The one major historical root of Functionalism, particularly in its quasi-teleological aspects, is Darwinism. The only significant psychological issue at stake between the intentionalists and the Wundtians was the introspective indecomposability of what was later called imageless thought. True, Brentano was also a presentative realist; and in this epistemological sense his acts "intended" physical objects. But then, the task of the structural historian is to show how superficial the similarity is rather than to exploit it.

GUSTAV BERGMANN.

State University of Iowa.

STONE, C. P., & TAYLOR, D. W. (Eds.) *Annual review of psychology*. Vol. 3. Stanford, Calif.: Annual Reviews, Inc., 1952. Pp. ix+462. \$6.00.

Volume 3 of the recently initiated *Annual Review of Psychology* largely continues the pattern established by the first two volumes. It consists of 17 reviews of psychological literature covering the 1950-1951 period. A total of 462 pages represents a continuing quantitative gain over the 330 pages in Volume 1 and the 389 pages in Volume 2.

This volume, like the first two in the series, must certainly be recognized as an important and useful contribution. In the opinion of the present reviewer, however, it still falls somewhat short of the goals originally set by the Editorial Committee. Its shortcomings seem to lie principally in the failure of certain of its contributors to adopt a sufficiently evaluative and critical point of view with regard to the literature surveyed. The result is that some of the chapters tend to read like a kind of super-abstract, with strictly factual accounts of major publications strung together more or less like beads on a string. In all fairness it must be said that so extreme a categorization clearly applies only to a minority of the chapters and that another minority offers a sharp contrast by presenting exceptionally well-organized and critical evaluations of the literature.

An excellent example of effective evaluation of a major field is provided by Henry W. Nissen and Josephine Semmes in their review of comparative and physiological psychology. They include a formal section entitled "The Status of Comparative Psychology." Their chapter is marked also by its comprehensive coverage of certain of the recent important European works on comparative problems, especially that associated with the names of Lorenz and Tinbergen.

The other four chapters falling generally within the framework of experimental psychology offer a variety of approaches. Harry F. Harlow's capable review of the learning literature is well organized according to major research problems but is lacking in any formal introductory or overview material. Some of this kind of material, however, is interpolated throughout the chapter. The chapter on vision by Harry Helson is likewise well organized and has, in addition, more explicit introductory material. Both of these chapters are written in a lively tone and from a critical point of view. In these respects the review of hearing by Wendell R. Garner suffers by comparison. It is written in a matter-of-fact style with little attempt at evaluation of large trends. This is not to question its technical competence. The review of somesthesia and the chemical senses by G. R. Wendt is written in a somewhat similar style but does have a superior organization of topics. The wealth of detail in this chapter suggests that it is intended more for the specialist in the sensory field than for the nonspecialist.

A strong experimental emphasis is pleasantly evident in three borderline chapters—borderline in the sense of belonging not quite within the field of experimental nor quite within that of clinical or applied psychology. These are the reviews of child psychology by Vincent Nowlis and Helen H. Nowlis, of social psychology and group processes by M. Brewster Smith, and of motivation by O. H. Mowrer.

The stimulating chapter by the Nowlises is written from a strongly critical point of view and stresses the need for more systematic theoretical orientation in research on children. The chapter by Smith is introduced by a useful and interesting discussion of the present status of social psychology as a distinct discipline. The evaluative and critical tone is maintained throughout the remainder of the chapter. The editors are to be commended for the addition in this volume of the chapter on motivation. In this review Mowrer adopts a highly selective organization and so is able to cover only 56 references. The chapter is highlighted by relatively long but interesting sections on the historical development of motivation theory and the present status of the "punishment" problem, which together account for more than one-half of the review.

Four chapters may be classified as distinctly "clinical" in nature. The review of the literature on personality is by H. J. Eysenck of the University of London. It is one of two reviews by European psychologists, marking the initiation of the planned policy to include a wider representation than would presumably be provided by exclusive use of American reviewers. Eysenck's chapter is well prepared, with a highly personal touch, and is spiced with frequent critical comments. A similar personal organization is present in the review of abnormalities of behavior written by Joseph Zubin. This chapter is noteworthy for its clear-cut statement of the restricted purpose of the review and the basis of selection of the articles reviewed.

The two reviews of methodological literature in clinical psychology are less obviously personal in their organization and bias. Ann Margaret's otherwise excellent chapter on psychodiagnostics is marred only by her seeming reluctance to make critical comments pertaining to specific articles. This comprehensive review contains an exceptionally well-prepared overview of the field and thoughtful strictures on general methodological defects. The chapter on psychotherapy by Victor C. Raimy likewise contains a good overview and is written in a lively and witty manner.

One of the most stimulating chapters in the volume is that prepared by Lloyd G. Humphreys on individual differences, a subject matter that seems to belong both to clinical and applied psychology. Humphreys' explicit purpose is "to sketch an outline of a functional psychology of individual differences." He organizes the review around

a number of selected research problems, indicates research needs as well as achievements, and does not hesitate to offer critical opinion of the articles reviewed. This chapter stands as a model of what can be achieved in an integration of the critical-evaluative and the pure abstracting functions.

Three reviews on distinctly applied subjects are offered. Clarence W. Brown and Edwin E. Ghiselli provide a top-notch chapter on industrial psychology, stressing research needs as well as critically evaluating achievements. Another competent review, surveying the literature on counseling, is contributed by William M. Gilbert. It should be noted that this chapter, carrying the title "Counseling: Therapy and Diagnosis," necessarily overlaps the two chapters on clinical methods (psychodiagnostics and psychotherapy). Perhaps the point of view is sufficiently different to justify the duplication, but it seems to this reviewer that a special editorial effort to delimit this area is indicated. The third applied review is that on educational psychology by John K. G. Elmgren, of the University of Gothenburg, Sweden. In certain respects this is the most unfortunate review in the volume. After a brief general discussion of the relationship between learning theory and educational problems, Elmgren launches into a technically competent but thoroughly humdrum and disconnected account of the various publications.

The single remaining chapter is a good, if somewhat condensed, review of statistical theory and research design by Quinn McNemar. The emphasis is on statistical *techniques*, as it should be, in spite of the fact that the title of the chapter refers to statistical theory. It is the opinion of the present reviewer that this kind of chapter stressing statistical and research techniques is today the most generally useful one to psychologists and should be expanded if possible in future editions.

Certain technical aspects of the 1952 volume deserve mention. One wonders why it should not be possible for the editors to devise a more logically planned arrangement for the order of appearance of the various chapters. No over-all organization is apparent in this respect, although short sequences of reviews of a similar type may be found. On the positive side, it is gratifying to note that, with but one exception, the bibliographies are now arranged alphabetically by author. Furthermore, the editors announce that on a trial basis in the next volume the reviewers will be given an opportunity to include if they will the full titles of journal articles. Reader opinion on this change is invited by the Editorial Committee, since it is stated that the extra space thus used will be in competition with that needed for exposition. (As a parenthetical suggestion in this connection, it may be ventured that considerable space could be saved if reviewers were discouraged from using the excessively long quotations—up to one-half page in length—that appear in several



of the chapters.) It is certainly to be hoped that these corrections of irritating defects of the bibliographies in the early editions will become permanent features of the series.

It is also to be hoped that the critical emphasis on research methodology and research needs, especially evident in several of the chapters in the 1952 volume, will become increasingly evident in future volumes. The present reviewer sees this feature as the most important long-range contribution of the *Annual Review of Psychology* to the development of psychology, as the inevitable splitting up of the discipline into separated but interrelated subject-matter areas is intensified. It will certainly be a major contribution if this series aids in the cross fertilization within psychology of research problems and research techniques.

MELVIN H. MARX.

*University of Missouri.*

McGEOCH, J. A., & IRION, A. L. *The psychology of human learning.* (2nd Ed., revised by A. L. Irion.) New York: Longmans, Green, 1952. Pp. xxii+596. \$5.00.

In its original edition, *The Psychology of Human Learning* was a book which no serious student of the field could afford to be without. It constituted the best single bibliographic source available and contained McGech's always carefully considered evaluations of the status of most of the important problems in the area. Now, after ten years, Irion has undertaken the gigantic task of revising the work, trying simultaneously to include the most important developments contributed by some three thousand articles which have appeared in the last decade and to maintain the theoretical position (really the lack of it) espoused by the original author.

To a degree, these two objectives turn out to be incompatible. The decade from 1940 to 1950 was distinguished more by theoretical contributions than by additions to empirical subject matter in the field of human learning. Such developments have been handled in the revision in two ways: (a) two new introductory chapters (on theoretical matters and conditioning) have been included; (b) at several places, discussions of animal experimentation have been introduced, where they are considered enlightening. These sections, however, do not fit in well with the ultra-positivistic position supplied by McGech's point of view. The program of theory making involves the development of postulate systems which, by a process of *deductive* elaboration, lead to testable theorems which can be confirmed or disconfirmed by observations on behavior. Such a discussion is very difficult to gear to an essentially *inductive* point of view like that of McGech's.

These last comments should not lead to the conclusion that Irion's revision of McGech's book has turned it into a discussion of learning

theory. They were made simply to show the kind of revision which ten years of development in the field of human learning has necessitated. Although they loom large in a discussion of *revisions*, they are less important in a description of the sort of book this is. It is still a scholarly, straightforward, factual summary of research in the field of human (chiefly verbal) learning. Considering the difficulties involved, Irion has done a remarkable job of maintaining the spirit of the original volume. And the reader's reaction to McGeoch and Irion will be about whatever it was to the original McGeoch.

A comparison of the literature cited in this book with that of the original edition provides us with an opportunity to assess the developments in the field of human learning that have occurred in the last ten years. Depending upon one's evaluation of the field in 1940, the result of such an analysis will be either comforting or distressing. For the book demonstrates that the mass of literature now available has not altered things markedly. A careful study reveals that only two new important contributors to the experimental literature (Irion and Underwood) have appeared in the last ten years. The authors leaned on most heavily in the original edition (Ebbinghaus, McGeoch, Melton, Thorndike) are still the important names in human learning. Similarly, the important topics are the same. The experimental problems treated in this book are very much like those discussed in the first edition. And the conclusions concerning matters of fact have changed little since 1940. Some changes in stress have occurred. The topics of reminiscence and the warm-up phenomena are given more attention in this book than in the previous one. This change has been chiefly at the expense of the topic of forgetting.

With one exception which will be pointed out below, the summary of the literature provided by McGeoch and Irion is excellent. And the question naturally arises as to the probable usefulness of a book of this sort. It is first of all *not* an undergraduate textbook. Too much background is taken for granted. For example, an understanding of operational definitions is assumed in the introductory chapter on theory. Furthermore, it shares with the original edition the characteristic of compressing a great deal of material into a fairly brief space. Undergraduates will find this volume hard going. Exactly the same features make the book a satisfactory (best available) text for graduate courses in human learning, although it has one chief drawback here too. This is the frequent reference to specific experiments (e.g., McGourty's master's thesis) as supporting some point without adequate description of the experiment. The student will find this mildly irritating or completely frustrating depending on his level of motivation.

As with the first edition of the book, McGeoch and Irion is a convenient source of summaries on a number of topics. The usefulness of the book in this way, however, will be somewhat restricted by the authors'

policy of citing the *earliest* experiment where two or more equally good ones make the same point. Unless there is a very high correlation between recency of experimentation and excellence of the experimental procedure, this policy could lead to the systematic elimination of the most recent work in the field. No methodical attempt has been made to evaluate the seriousness of the result of this fact; but the reader should be aware that the most recent article cited by McGeoch and Irion is not necessarily the last word on the subject.

All in all, this revision lives up to the reputation of the original edition. The limitations, which have been pointed out above, do not weigh heavily against the fact that we now have a fairly up-to-date summary of an important field of psychology.

GREGORY A. KIMBLE.

Duke University.

KELLY, E. LOWELL, & FISKE, DONALD W. *The prediction of performance in clinical psychology*. Univer. of Michigan Press, 1951. Pp. xv+311. \$5.00.

During the Spring of 1946, the Veterans Administration completed its plans for training large numbers of clinical psychologists, . . . the Veterans Administration decided also to institute a program of research designed to make possible a more effective selection of trainees than might otherwise occur. . . . Early in the summer of 1946, the University of Michigan was approached as to its willingness to accept the responsibility for the direction and coordination of such a research program. . . .

. . . The over-all design of the project was as follows: in 1947 and 1948, several hundred college graduates seeking admission to or just entering the four-year VA training program in clinical psychology in some 40 universities were evaluated by a wide variety of techniques, and predictions were made concerning their probable success in training and their future professional competence. . . .

*The Prediction of Performance in Clinical Psychology* is an account of a necessarily elaborate study which is remarkable both for the candor of its presentation and the breadth of its content. All of the various phases, which required a total of five years, are described in useful detail. In essence clinical psychologists have employed the most promising of their devices in an attempt to predict the responses of students to training in clinical psychology. Although the most hoped for goals in this study were not realized, the scope of this report makes it an important chapter in our literature of applied psychology.

This study has succeeded in providing us with additional reassurance that we can make useful predictions of a student's achievements with respect to the more formal academic aspects of graduate training. The results for the primarily clinical criteria do not provide a means for useful prediction of the development of clinical students as clinicians.

Considering the intangible nature of clinical proficiency and the general lack of satisfactory evidence concerning the efficacy of clinical procedures per se, this failure is not surprising. Accordingly, the following comments are neither criticisms nor suggested improvements; they merely provide a context for a brief discussion of the general nature of some features of this interesting work.

The author's report describes two major emphases. One has to do with the development of measurements and indices which are of predictive value; the other has to do with the development of measurements and indices which are criteria for the development or attainments of student clinical psychologists. From the standpoint of the predictive variables, the undertaking was unique in both its extensivity and intensity. Tests of mental ability, of various achievements, and of vocational interests, as well as personality attributes, and judgments and ratings based on subjective tests, interview situations, and sociometric procedures were all employed. It is probable that the various clinical psychologists who participated in this undertaking (the list of participants looks like a "Who's Who in Clinical Psychology") applied their clinical skills and insights unsparingly to this task.

The emphasis on the criteria is frank and conscientious. As a matter of fact the sections which have to do with the development of criteria could serve as a reference for the current definition of clinical psychology. It seems to this reviewer, however, that the manner in which the criterion problem was handled was determined by one major point of view. Specifically, it appears that the investigators felt either that they knew or ought to know the desirable characteristics of a clinical psychologist. (This quite natural bias is explicitly acknowledged by the authors.) This bias led to the development of criteria which emphasized "goodness" among the developing clinical psychologists rather than "differences" between them. This necessarily meant that attempts to make useful predictions for the developing students were limited to a restricted number of currently acceptable aspects of "goodness." As a consequence no comprehensive prediction of "differences" is attempted.

Perhaps the major clinical criteria were ratings of diagnostic ability, therapeutic ability, and desirability as an employee. Although it appears that such ratings may be made with fair consistency, one is inclined to wonder how much the meaning of these ratings could vary from school to school and whether or not there are internal consistency analyses to indicate that all of the items on the rating scales belong on the same continuum. It is possible that the items on the rating scales are very well placed from the standpoint of general desirability, i.e., "goodness." It is also possible that from a behavioral standpoint they do not belong on the same scale.

It appears that individuals from different schools were combined for the purpose of statistical analysis. Nevertheless, it is known that



schools differ in the manner in which they select students and the manner in which they train students. This suggests a possibility that for some variables the magnitude or the direction of correlation may vary somewhat from school to school. It also seems possible that a respective correlation for a sample of schools and for a sample of individuals within a school might be different. Although the writers were obviously and most properly addressing themselves to trends which obtain for all VA clinical trainees, it is quite possible that the essential nature of such trends could be found by summarizing the possible trends found separately in individual schools (or in groups of similar schools) and that such a summary could present a more optimistic picture than one revealed for the total sample of all schools. As a matter of fact, it may be difficult for some readers to decide whether they should attempt to generalize to schools or to individuals. An attempt was made to eliminate school differences from some of the variables and it is possible that this reduced the heterogeneity of the measures for the total group, thus reducing the probability of a high correlation.

The authors have been conservative in all of their claims and have not made predictions based on weighted combinations of various measures; presumably, they sought to avoid a spuriously high indication of relationship. It seems possible, however, that some weighted combination of the data could be profitable. It is hoped that for a restricted portion of the sample multiple predictions would be prepared and then cross-validated on some similar but independent portion of the sample.

The present review does not presume to provide a detailed commentary on the methods and rationale of the study, nor is it intended to propose alternative approaches. Nevertheless, it is quite possible that many readers would like to know why alternative analytical procedures were not employed or why a more diversified attempt at the criterion problem was not attempted. Accordingly, the negative implications of the present study should not be overemphasized. Perhaps meager results should never be taken as an indication that positive results are not possible. Certainly the present study should not be taken as a basis for claiming that important aspects of clinical proficiency cannot be anticipated from the use of these or other predictive measures. On the contrary, many aspects of the present study provide a useful and encouraging exploration.

J. R. WITTENBORN.

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